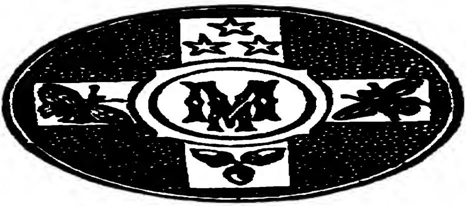
THE BEGINNINGS OF LIFE.



THE BEGINNINGS OF LIFE:

BEING

SOME ACCOUNT OF THE NATURE,

MODES OF ORIGIN AND TRANSFORMATIONS

OF

LOWER ORGANISMS.

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THE BEGINNINGS OF LIFE.

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IN order to ensure the more general acceptance of the conclusions concerning the nature and origin of Living Matter to which the experimental evidence

has compelled us to arrive, it remains for me to show, how the facts, to which M. Pasteur and others call attention in support of the atmospheric 'germ theory,' are capable of quite a different interpretation; and how (in the presence of new facts) the initiation of fermentative processes is even more explicable from a point of view which they almost utterly neglect, than it is from their own standpoint. M. Pasteur's celebrated experiments with fermentable fluids which had been boiled in flasks with long, narrow, and bent necks; those where the fluids were exposed to the air of various localities; and those in which previously sterile fluids had been rendered fertile by an inoculation with atmospheric particles—all these, so far from being conclusively in favour of his own doctrines, are even much more explicable in accordance with the wider doctrines concerning fermentation held by Baron Liebig.

Two considerations—both of them almost ignored by M. Pasteur and his followers—require to be continually borne in mind in interpreting the results of any experiments bearing upon the cause of fermentation and upon the possibility of the *de novo* origination of organisms. They are these:—

1. That dust filtered from the atmosphere has not been proved to contain living *Bacteria*, though it is well known to contain a multitude of organic particles, which, in accordance with Liebig's hypothesis, are capable of acting as ferments in the presence of water.

2. That, in accordance with the views of evolutionists, 'life' may be considered to represent the sum-total of properties displayed by certain kinds of organic matter; and that these higher properties may be deteriorated or rendered non-existent by an amount of heat which may not be adequate wholly to decompose the organic matter itself.

The first is a very important consideration. It should be clearly understood, that even if we could demonstrate the presence of *Bacteria* in the atmosphere, this alone would not be enough. The panspermatists ought to be able to demonstrate the existence of universally disseminated living *Bacteria*, and therefore they may be fairly asked to show—what as yet they have never attempted—that *Bacteria* are well capable of resisting such an amount of desiccation as must be involved by their presence for an indefinite time in the atmosphere even of the hottest and driest regions of the earth. For organic substances in solution do not only putrefy in moist weather or moist climates; they putrefy, on the contrary, most rapidly and surely when the temperature is high, and quite irrespectively of the amount of moisture contained in the atmosphere. The capability of resisting the effects of desiccation—the possession of which, by *Bacteria*, is so necessary for the truth of M. Pasteur's argument—ought to have been shown by scientific evidence to be a real attribute of such organisms; though it seems, on the contrary, to have been

assumed to exist by nearly all those who have taken part in the controversies concerning the possibility of 'spontaneous generation.' This error may again be ascribed to the misguiding influence of a treacherous analogy. Whilst it may be true that certain seeds and spores, and also that Rotifers, 'Sloths,' and some Nematoids are capable of resisting the influence of a prolonged exposure to desiccating influences, it may well be asked, whether the same fact necessarily holds good for organisms such as *Bacteria*, having no chitinous or other envelopes to protect them, and which are merely minute fragments of naked protoplasm. Having elsewhere [1] shown how far presumptions had stolen a march upon established facts, in reference to the supposed possession of a similar property by the Free Nematoids, my eyes were opened to the reality of this uncertainty with regard to *Bacteria*. It is, however, no easy matter definitely to prove or to disprove the possession of this property by organisms so minute as *Bacteria*, and therefore so difficult to identify. If dried *Bacteria* are added to a drop of a suitable solution—similar to that in which they had been bred—it soon becomes quite impossible to distinguish those which have been added from those which arise in the fluid [2]. Taking into consideration the

[1] 'Philosophical Transactions,' 1866, pp. 616–619.

[2] And similarly if we introduce dried *Bacteria* into a solution which will nourish them, although it had previously no tendency to breed them *de novo*, and *Bacteria* are subsequently produced, we cannot safely affirm

fate of other simple organisms, however, it is by no means improbable that they should be killed even by a short desiccation. I have found, for instance, that desiccation for half-an-hour in a room at the temperature of 65°F suffices to kill all the larger, naked, lower organisms with which I have experimented, including Amœbæ, Monads, Chlamydomonads, Euglenæ, Desmids, Vorticellæ, and other Ciliated Infusoria.

And as a result of his more recent experiments, Dr. Burdon Sanderson[1] has definitely come to the conclusion, not only that 'the germinal particles of microzymes are rendered inactive by thorough drying without the application of heat,' but also that 'fully-formed *Bacteria* are deprived of their power of further development by thorough desiccation.' The amount of desiccation induced being merely that occasioned by keeping them for two or three days in an uncovered condition exposed to a temperature of 104°F, which is, of course, a far lower temperature than that to which the *Bacteria* and their germs would be exposed in the atmosphere, in many hot countries, where putrefaction, nevertheless, occurs with amazing facility.

Certain other evidence also seems to speak most authoritatively against the supposition that the air con-

that these are the legitimate descendants of the dried *Bacteria* which were sown, because we cannot be sure that the dried mass may not have acted as a mere dead ferment, which by its *motor-decay* determined a *de novo* production of *Bacteria* in the test-liquid.

[1] Thirteenth Report of the Medical Officer of the Privy Council, p. 61.

tains any notable quantity of living *Bacteria*, or of their germs, whether visible or invisible. I have always found that a simple solution of ammonic tartrate, which has been placed—without previous boiling—in a corked bottle of greater capacity, will become turbid in two or three days, owing to the presence of myriads of *Bacteria*; whilst a similar solution, previously boiled, may remain for ten days, three weeks, or more, without showing the least trace of turbidity, although the open neck of the bottle or flask in which it is contained may be covered only by a loose cap of paper. And yet, at any time, in order to make this fluid become turbid in twenty-four to forty-eight hours, all that one has to do is to bring it into contact with a small glass rod which has just been dipped into a solution containing living *Bacteria*[1].

If we find that an eminently inoculable fluid will remain for two or three weeks, or perhaps more, in contact with the air without becoming turbid, though it will always become turbid in two or three days if brought into contact with living *Bacteria*, what can we conclude, but that living *Bacteria* are not very common in the atmosphere? These most striking facts can be easily verified by other observers[2].

On this subject also, I am glad to find that my

[1] The solution during the whole time being exposed to a temperature of 75° to 85° F.

[2] Somewhat similar facts were indeed first recorded by Prof. Cantoni, 'Rendiconti di Lombardo,' Nov. 25, 1869.

conclusions have been independently confirmed by the results of the recent experiments of Dr. Burdon Sanderson[1]. Speaking of 'Pasteur's solution,' with which he had been working, he says:—'No amount of exposure has any effect in determining the evolution of microzymes. This conclusion, although it is in complete accordance with what we have already learned as to their relations, both in the visible and invisible state, to moisture, is of such importance that it seemed necessary to establish it by special experiments.' The following is the most striking of the experiments which were made with this object in view. 'January 7.—The bent glass tube for the absorption of carbonic acid by potash, known as Liebig's bulbs, was heated to 200°C and filled with boiling test solution. It was then attached, by a vulcanite connector which had been previously boiled, to an aspirator. During the following week air was drawn through it for a few hours daily. On the 23rd there were numerous *Torula* cells with submerged tufts of mycelium in the liquid, especially in those bulbs to which the air had access first, but no trace of microzymes. The result shows in the most striking manner not only that *air is entirely free from living microzymes*, but that the activity of the development of penicillium is in proportion to the degree of exposure.'

M. Pasteur, Prof. Lister, Prof. Huxley, and others,

[1] Loc. cit., p. 59.

state that fermentable fluids which have been boiled, will not undergo fermentation in vessels whose necks have been many times bent, or in those into whose necks a plug of cotton-wool has been inserted during the ebullition of the fluid which they contain. And they say that organisms are not found in such cases because the hypothetical atmospheric 'germs,' from which the *Bacteria* and *Vibriones* of infusions are usually produced, are arrested either in the flexures of the tube or in the cotton-wool. It is obvious, however, that if this explanation be the correct one, the preservation should be equally well marked in all cases—quite irrespectively of the amount of albumenoid or other nitrogenous material which the fluid contains. Any exceptions to the rule should at once suggest doubts as to the validity of the explanation.

Yet it was shown[1] in 1865 by M. Victor Meunier, that whilst some fluids were preserved after having been boiled in a vessel of this kind, others, when submitted to the same treatment, speedily became turbid from the presence of *Bacteria* and other organisms[2]. By these experiments he ascertained that

[1] 'Compt. Rend.' t. lxi. p. 1060.

[2] When boiled solutions, containing mannite, with a little nitrate and phosphate of ammonia, were employed, they always remained sterile. Similar negative results followed the employment of ox-gall. Of three decoctions of beef with which M. Meunier experimented, the two stronger of them were found to contain swarms of *Bacteria* in about twelve days. Of three other flasks containing boiled urine, only one was productive.

strong infusions frequently changed, whilst weak ones might be preserved; and that even a strong infusion might be prevented from undergoing change, if the period of ebullition were sufficiently prolonged. Prof. Cantoni also found that *Vibriones* were plentifully produced within such flasks when very strong organic fluids were employed, and when the daily temperature of the air was not less than 77°F [1].

The fluids most frequently employed by M. Pasteur were yeast-water, yeast-water sweetened by sugar, urine, infusion of beet-root, and infusion of pear.

Taking urine as a fair example of such a fluid, I have found that the statements of M. Pasteur and of Prof. Lister are perfectly correct. This fluid may generally remain for an indefinite period in such vessels without becoming turbid, or undergoing any apparent change. The same is generally found to be the case with an infusion of turnip; and occasionally an infusion of hay may be similarly prevented from undergoing fermentation. On the other hand, if the turnip-solution be neutralized by the addition of a little ammonic carbonate, or liquor potassæ; or, better still, if even half a grain of new cheese be added to the

[1] After speaking of the vital resistance to heat of *Vibriones*, he says ('Gaz. Med. Italiana-Lombardia,' serie vi. tom. I. 1868):—'The temperature at which the production of *Vibriones* ceases in an organic solution varies with the quality of the organic matter dissolved in it, with the quantity of air enclosed in the flasks together with the solution, and also (and more notably) with the temperature of the air in which the flasks are kept after being heated.'

infusion before it is boiled, then I have found that the fluid speedily becomes turbid, owing to the appearance of multitudes of *Bacteria*[1]. In an infusion to which a fragment of cheese had been added, I have seen a pellicle form in three days, which, on microscopical examination, proved to be composed of an aggregation of *Bacteria*, *Vibriones*, and *Leptothrix* filaments. Again, a mixture of albuminous urine and turnip-infusion has rapidly become turbid in a vessel of this kind owing to the presence of multitudes of *Bacteria*, and so also has a mixture containing one-third of urine with two-thirds of infusion of turnip[2].

Other infusions have been boiled for ten minutes in a vessel with a horizontal neck two feet long, into which, during ebullition, a good plug of cotton-wool had been carefully pushed down for a depth of twelve or fourteen inches, and cautiously increased in quantity during the continuance of the ebullition. Immediately after the withdrawal of the heat, the plug of wool was made more dense, and the outer portion of the tube was rapidly filled up with the same material to the whole depth of twelve or fourteen inches. When preserved in such a vessel, a specimen of urine remained unchanged; a hay-infusion also underwent no apparent

[1] Of course germs may be in the minute fragments of cheese, just as they may be in the organic infusion itself. All, however, must be killed during the process of ebullition, and the subsequent results must be ascribed in part to the superior molecular mobility still remaining in the particles of boiled cheese.

[2] See numerous experiments recorded in *Appendix* C.

alteration; whilst a very strong infusion of turnip became turbid in five days, and ultimately showed a large quantity of deposit[1].

The results not being uniform, the explanation offered by M. Pasteur and others, as to the cause of the preservation of the particular fluids with which they experimented, is at once rendered doubtful. More especially is there room for doubt on this subject when the result of the experiment can be predicated beforehand, within certain limits, as I have found, according to the nature of the fluid employed. If the organisms in these experiments all proceed from pre-existing germs, which can be filtered from the air by a certain mechanical contrivance, then, if it be alleged that it is on account of such filtration that certain boiled fluids do not change, all fluids placed under these conditions ought, on this theory, to be similarly preserved. Exceptional cases cannot be accounted for on this hypothesis. To others, however, who say that organisms are capable of arising *de novo*, and that fermentation can be initiated without the agency of living things, the above facts appear quite natural. They think that the more the nitrogenous or protein materials contained in a solution are complex and abundant, the more is the solution fitted to undergo

[1] See *Appendix* C. p. xxxiii. These are the only experiments which I have performed with the very long plugs of cotton-wool, though in other previous trials with plugs about $1\frac{1}{2}''$ long, I have several times obtained positive results.

such fermentative changes as are accompanied by the *de novo* origination of living things. The above-mentioned apparently exceptional results are, therefore, just as compatible with the notions of M. Liebig and his school, as they are antagonistic to those of M. Pasteur. Certain simpler fluids do not undergo change, whilst others of a more complex description, under the influence of similar conditions, do ferment.

The complete untenability of M. Pasteur's explanations are, however, best revealed by having recourse to a series of comparative experiments, in which portions of the same fluid are boiled for an equal length of time in vessels of different kinds, and are subsequently submitted, in a water-bath, to the influence of the same temperature. Owing to the different behaviour of the same fluids under different conditions, we are enabled to draw some very important conclusions; but from the different behaviour of different fluids under these respective conditions, we are enabled to eliminate many of the explanations of M. Pasteur and others, whilst at the same time facts are revealed of the most decisive nature, bearing upon the relative merits of the two doctrines as to the cause of fermentation and putrefaction [1].

Such experiments show quite conclusively that M. Pasteur's explanations are altogether inadequate to account for the occasional preservation of boiled fluids in bent-neck flasks. The preservation, far from being

[1] *Appendix* C is a record of experiments of this kind.

universal, is only occasional, and whether this or an opposite fate awaits the different fluids, is shown, as already stated, to be almost wholly dependent upon their nature. The comparative .experiments not only lend no countenance to M. Pasteur's theory, that fermentation cannot be initiated without the agency of living ferments—they are, on the contrary, wholly opposed to this restriction.

The plug of cotton-wool, or the narrow and bent tube, may, it is true, protect the boiled fluid from subsequent contact with living 'germs;' but that the fluids do not undergo change on account of such deprivation cannot be safely affirmed, when the same means would also filter from the fluid some of the multitudinous particles of organic matter (dead), which the air undoubtedly contains, and which may act as ferments. It must be remembered that the main object of M. Pasteur's investigation was to determine whether fermentation took place under the agency of mere dead nitrogenous matter, as Liebig and others affirm, or whether it is only initiated by living organisms, as he himself supposes. Obviously, therefore, the same filtration which purified the air from any living organisms would filter from it its nitrogenous particles, which are the other possible ferments: so that no conclusion could be drawn from such experiments more favourable to the one than to the other of these two hypotheses. All that could have been safely affirmed was, that by boiling the fluid, and then protecting it

from subsequent contact with everything that could act as a ferment, fermentation would not take place.

But now, even this cannot be truly affirmed to be a general rule. Some infusions still preserve a first degree of fermentability even after boiling, whilst others are reduced by this process to the second degree of fermentability. The latter, unlike the former, are unable to initiate changes by virtue of their own inherent instability: molecular re-arrangements require to be set on foot in them by contact with an unstable substance (dead or living) which is itself undergoing change [1].

That such is the correct explanation of the reason why *some* fluids do not ferment in bent-neck flasks, seems obvious from the discordant results obtained in many other experiments, after the free admission of uncalcined air to the fluids which had been boiled. The fluids were deprived of their virtues in some cases

[1] In the face of these rival doctrines of fermentation and the similarly unsettled state of our knowledge concerning all the modes of origin of *Bacteria*, it may now be seen how rash and unscientific was the assumption at once indulged in by M. Pasteur and his followers (who are constantly trumpeting the logical acumen of their chief), that because the contact of atmospheric particles with the fermentable fluids could be shown to be the cause of their fermentation, therefore living germs must have existed amongst these atmospheric particles. As I have previously pointed out, such a conclusion could only be rendered valid on the strength of the postulate that 'all life proceeds from pre-existing life,' that is to say, on the strength of a postulate which settled beforehand the problem which their investigations were destined to solve. Many of the remarks of Dr. Burdon Sanderson (loc. cit.) concerning the cause of the zymotic qualities of water seem to me to be open to the same criticism.

by the heat to which they had been subjected, so that whether they underwent change or not, may have depended upon the accidental presence or absence of suitable unheated organic fragments in the air admitted to the fluid. If germs were as omnipresent as they have been represented to be, such fluids ought always to have undergone change. Owing to facts of this nature, M. Pasteur[1] came to the conclusion that 'germs' are not so universally distributed as they had been supposed to be by Bonnet and Spallanzani[2]. The unprejudiced inquirer, however, will perceive that M. Pasteur was entitled to come to no such conclusion concerning germs which was not equally applicable to minute fragments or *débris* of organic matter floating in the air. And, similarly, the evidence which he adduces with regard to the diminution in the number of the fertile flasks when they were filled with some of the still air of the caves of the observatory, or with the air of some high mountain regions[3], far away from the haunts of men, had no bearing upon the distribution of germs which was not equally applicable to that of dead organic particles. Such evidence, therefore, was valueless for settling between the rival doctrines of fermentation, and could not possibly help us to decide whether living or dead ferments were necessary. Dead organic particles would sink in still air in the same manner as

[1] 'Ann. de Chimie et de Physique,' 1862, p. 71.
[2] Loc. cit., pp. 75 and 76. [3] Loc. cit., pp. 83 and 84.

living organisms[1]; and similarly, dead organic particles have been shown to be less and less numerous in the atmosphere in proportion to the elevation obtained[2]. In these latter experiments M. Pasteur made use of yeast-water (alone or sweetened), and of urine—all three of them fluids, which, after having been boiled, are apt to possess only the second degree of fermentability. So that when we find M. Pouchet, in concert with MM. Jolly and Musset, repeating these experiments, with the sole difference that they took strong infusions of hay—which experiment has almost invariably shown to possess the first degree of fermentability—and that all their flasks, after a time, yielded organisms from whatever mountain elevation the air had been taken, this combined evidence tends most strongly against the view of M. Pasteur. As the germs in the fluids and in the flasks, in each set of experiments, had been previously destroyed by ebullition, and since in each set, also, air of the same character

[1] The subsidence of the atmospheric particles has been demonstrated by Professor Tyndall ('Proceedings of Royal Inst.' 1870, p. 11). After speaking of experiments in closed flasks, in which the air has been either calcined or filtered, Gerhardt ('Chimie Organique,' t. iv. p. 545) says:—'Si dans les premières expériences l'air calciné ou tamisé s'est montré beaucoup moins actif que l'air non soumis à ce traitement, c'est que la chaleur rouge ou le tamisage enlève à l'air non seulement les germes des infusoires et des moisissures, mais encore les débris des matières en décomposition qui y sont suspendues, c'est-à-dire les ferments dont l'activité viendrait s'ajouter à celle de l'oxygène de l'air.'

[2] See M. Pouchet's 'Nouvelles Expériences sur la Génération Spontanée,' &c., p. 69.

had been admitted to the boiled fluids, the different results seemed to show that fermentation or non-fermentation, in such cases, depends wholly upon the quality of the fluids employed.

Other evidence which is so much vaunted by M. Pasteur and his supporters, as to the possibility of inducing fertility in previously sterile flasks by the addition of a portion of asbestos, containing the solid particles filtered from the atmosphere [1], is also equally valueless for confirming the proposition that fermentation is only capable of being initiated by living ferments. The same asbestos which *may* contain living germs or organisms, does undoubtedly contain many decomposable particles and fragments of organic matter [2]. The previously barren solution may therefore be rendered fertile by the mere addition of those portions of unstable organic matter, whose molecular mobility has not been wholly impaired by the agency of heat, so that they are still capable of initiating fermentative changes. This view is strengthened, as M. Pouchet has pointed out, by the fact that in these cases, instead of meeting some of the *various* kinds of organisms which are considered to have representatives in the air, and whose spores or ova may be supposed to have been sown, it is often merely *Bacteria* which are encountered. And these differ in no respect from those that may present themselves in a somewhat similar infusion, which

[1] Loc. cit., p. 40.
[2] See M. Pouchet's 'Nouvelles Expériences,' 1863, pp. 94–107.

has undergone change in a closed flask without any such hypothetical sowing of living spores or germs. It is more especially important to bear this consideration in mind, seeing that portions of organic matter can always be easily demonstrated amongst such atmospheric dust; whilst living *Bacteria* have been shown by Cantoni, Sanderson, and myself to be almost always absent[1].

The views hitherto expressed with reference to the causes of fermentation and putrefaction, and concerning the interpretations which M. Pasteur's experiments are capable of receiving, seem to derive all the additional support that can be needed, from the results of my own experiments with boiled fluids in sealed flasks from which all air had been expelled.

Some of a given fluid being taken and divided into three parts, each portion is placed in a separate flask, in which it is boiled for a period of ten minutes. One of the flasks (A) is provided with a long and bent neck, so that the air which re-enters is deprived of its germs and organic particles; another (B) has only a short neck, and to this, the access of germs and organic particles is freely permitted till the fluid has become cool, when the neck of the flask is hermetically sealed; whilst the last (C) is sealed during ebullition, after all air has been expelled. Now, if Pasteur's theory of fermentation, and the prevalent notions concerning the universal distribution of 'germs' throughout the

[1] On what other supposition can one explain the results of *Exps.* lvii–lxv?

atmosphere were true, it might be expected that the fluid in B would always rapidly change; that that in A would always remain pure; and that the fluid in C would, similarly, undergo no alteration. The facts, however, are quite the reverse: if a properly prepared turnip-infusion be employed, the fluid in A will almost always remain unchanged; that in B will sometimes rapidly change, and at other times will remain quite pure; whilst that in C will almost invariably become turbid in from two to six days. So that even if it were not the case that some fluids, different from those used by M. Pasteur, will almost invariably undergo change in bent-neck vessels, his explanation of the cause of the preservation would have been altogether upset by the fact that *some of the very fluids which remain pure in the bent-neck apparatus will become fœtid if shut up in vacuo*[1]. If M. Pasteur's theory were true, exclusion of all air from the flask should prove just as efficacious in protecting the fluid as any process of filtration. And the fact that some of the very fluids which are protected as long as they are in contact with air devoid of particles, can be made to ferment and swarm with living things through the mere expulsion of this purified air[2], is the death-blow to M. Pasteur's theory, and one of the strongest proofs of the occurrence of Archebiosis.

The cause of the change in the latter case seems also

[1] See *Appendix C, Exps.* vii-ix, xiii-xv, &c.

[2] This has been done on several occasions. See *Exps.* ix and xv, and xxiii and xxxvi.

pretty evident. Germs and atmospheric particles being equally got rid of in both modes of experimentation, the great difference between them is that the weight of the atmosphere is also got rid of in my experiments—the fluids being contained *in vacuo*. But, as I have already pointed out [1], it has been ascertained by Mr. Sorby, that pressure undoubtedly influences 'chemical changes taking place slowly,' and such as are therefore 'probably due to weak or nearly counterbalanced affinities;' and it has also been shown that 'pressure will more or less influence such chemical actions as are accompanied by an evolution of gas, so that it may cause a compound to be permanent, which otherwise would be decomposed.' But, if increase of pressure retards, a diminution of pressure may be expected to facilitate such chemical changes, so that one can only explain the results which I have obtained, on the ground that many boiled fluids, which will not undergo change when protected from the influence of atmospheric particles (living or not living) at the same time that they are subjected to ordinary or increased pressure, will, on the contrary, pass through such changes when pressure is diminished, by the fluids being preserved *in vacuo* [2]. It is not pretended that this is a rule applicable to *all* fermentable fluids—far from it [3].

[1] See vol. i. p. 350.

[2] On this subject, see vol. i. pp. 418–420.

[3] I very soon convinced myself, in fact, that diminution of pressure exercised very little effect over the changes which take place in solutions

Diminution of pressure seems, however, to be a very potential cause of change in some fluids. The extent to which changes of a fermentative character can progress in the absence of atmospheric oxygen, is also evidently subject to much variation, in accordance with the nature of the dissolved fermentable substances.

Thus, in accordance with the doctrines of Baron Liebig, my experiments, as well as those of many other investigators, tend to show that fermentative and putrefactive changes are merely processes of chemical rearrangement, which frequently take place—as it were 'spontaneously'—owing to the inherent instability of certain nitrogenous compounds in the presence of free oxygen. My experiments have, however, also revealed the additional fact that, under the combined influence of a moderate heat and diminished pressure, some fluids will undergo fermentation even in closed vessels, from which all air has been expelled. And, at the same time, they compel us to believe that the lowest organisms, when present, are often mere concomitant products (some of which have arisen *de novo*), rather than invariable or necessary causes of the fermentative changes.

of ammonic tartrate and sodic phosphate. The facts recorded by Dr. Sanderson (loc. cit., p. 54) as to its lack of influence over 'Pasteur's solution,' is therefore quite what might have been expected. It was a mistake to suppose that I considered diminution of pressure to be invariably favourable to the occurrence of fermentable changes in all fluids capable of undergoing this change.

Turning now, however, to another aspect of the question, and accepting the fact that specks of living matter, which speedily develop into the simplest organic forms, are capable of arising *de novo*, we are at once met with the query—Out of what materials has this living matter been developed, and what were the steps of the process? In my experiments I have employed, (1) Simple infusions containing organic matter; (2) Saline solutions to which, in addition to any unknown organic impurities, a fragment of organic matter had been purposely added; and also (3) Saline solutions to which nothing was added, but which may have contained accidental organic impurities.

So far as the organic infusions are concerned, we know that these contain complex colloidal molecules in solution, which, though altered and more or less degraded in quality by the influence of the high temperatures, have probably still retained some of their characteristic properties. So that, after a time, under the continued influence of heat, light, and other agencies, new combinations may have been brought about amongst these mobile compounds, till continuous changes of a fermentative character were initiated, which resulted in the coincident production of specks of living matter. These are supposed to be formed by the occurrence of new combinations amongst the colloidal molecules themselves, of a kind similar to those which must occur when the elements of ammonic cyanate assume the more complex arrangement which converts them into

urea. The resulting compounds, being insoluble, separate from the solution in the form of minutest specks of living matter, which speedily develop into this or that kind of primordial organism. Such higher compounds might be considered to correspond to the 'physiological units' whose existence Mr. Herbert Spencer[1] postulates in order to explain the various phenomena coming under the head of 'organic polarity.'

After pointing out that the phenomena cannot be accounted for if we suppose such 'polarity' to be possessed by the ordinary chemical constituents of living things—by their mere molecules of fibrine, albumen, or gelatine; and also that the phenomena are even more inexplicable if we assume that such polarity is the property of any kind of morphological unit (such as a 'cell') existing in living things, Mr. Spencer adds:— 'If then this organic polarity can be possessed neither by the chemical units nor the morphological units, we must conceive it as possessed by certain intermediate units, which we may term *physiological*. There seems no alternative but to suppose that the chemical units combine into units immensely more complex than themselves, complex as they are; and that in each organism the physiological units produced by this further compounding of highly compound atoms, have a more or less distinctive character[2]. We must con-

[1] 'Principles of Biology,' vol. i. p. 182.

[2] For further suggestions with regard to these physiological units, the reader may consult the Appendix (p. 486) to Mr. Spencer's 'Principles of Biology.'

clude that in each case some slight difference of composition in these units, leading to some slight difference in their mutual play of forces, produces a difference in the form which the aggregate of them assumes.'

When we suppose—not voluntarily, but on account of facts otherwise inexplicable—that some such higher combinations arise 'spontaneously' amongst the molecules of organic matter contained within hermetically sealed flasks, it must not be thought that we are appealing to processes which are new and previously unrecognized. On the contrary, chemists are perfectly familiar with such 'spontaneous' combinations taking place amongst the molecules of various complex substances. And, occasionally, these changes result in the formation of isomeric compounds, differing from those previously in existence by reason of their greater molecular complexity, just as 'physiological units' are supposed to differ from the higher colloid molecules. As instances of this kind of change, we may call attention to the following well-known facts. Cyanate of ammonia ($C N^2 H^4 O$) in aqueous solution is converted by the aid of heat, or 'spontaneously' when left to evaporate at a low temperature, into urea ($C^2 N^2 H^4 O^2$). Cyanic acid ($C N H O$) in aqueous solution is spontaneously converted into cyanelide ($(C N H O)^x$). Cyanimide ($C N^2 H^2$) at 150° C is converted into cyanuramide ($C^3 N^6 H^6$). Common aldehyde ($C^2 H^4 O$) in aqueous solution, with a mere trace of oil of vitriol, is changed into an oily aldehyde ($C^6 H^{12} O^3$). Anhydrous sulphuric

acid (SO_3) soon after preparation becomes converted into a body of the same percentage composition, though of higher melting point. There are, moreover, very good reasons, approved by chemists, for believing that nitric peroxide gas, NO^2, when at a low temperature, becomes N^2O^4; that the composition of hydric acetate (vinegar) is $C^4H^8O^4$ in the liquid state, but $C^2H^4O^2$ in the gaseous state; and that bitter almond oil, in presence of certain reagents, is capable of doubling itself (C^7H^6O into $C^{14}H^{12}O^2$), even with change of chemical constitution—for $C^7H^6O = (C^7H^5O)H$, or hydride of benzoyl, whilst $C^{14}H^{12}O^2 = (C^7H^5O)(C^7H^7)O$, or benzoate of benzyl. Strictly analogous, also, to these reactions between similar molecules are those in which two or more dissimilar molecules coalesce—as when two oxides, two chlorides, two cyanides, two sulphates, &c., unite to form double oxides, double chlorides, double cyanides, double sulphates, &c. Similar unions are also known to take place between organic or carbon compounds, e. g. cyanamide (CN^2H^2) and glycocol ($C^2H^5NO^2$), which are both obtainable synthetically, combine, when present together in aqueous solution, to yield glycocyamine ($C^3H^7N^3O^2$), a body homologous in properties and composition with kreatine[1].

If we are asked to explain why, or in what manner,

[1] My attention has kindly been called to these synthetic changes by Mr. Temple Orme, of University College, to whom I have been much indebted for information of this kind.

colloid molecules combine and undergo molecular rearrangements leading to the formation of those insoluble compounds which separate from the solution in the form of specks of living matter, we can only give expression to our profound ignorance on the subject. At the same time, however, we can express only the same ignorance as to the reason why any of the other more simple chemical changes occur, as it were spontaneously. The fact that the new products do make their appearance in the latter set of cases, leaves no doubt as to the conclusion that they have originated by molecular rearrangements which have taken place amongst the pre-existing elements. And in the face of the evidence which has been adduced, it seems to me almost equally certain that the organisms which have been found in some of our flasks must have developed from specks of living matter, which had themselves originated from a molecular rearrangement and combination occurring amongst the colloid molecules of the solution. Just as the colloids themselves have been produced as a result of the molecular interaction of substances having a simpler composition, so may living matter be produced through the molecular interaction of colloids under the influence of heat and other physical agencies. We cannot explain why such interaction and molecular rearrangement should take place amongst colloidal molecules, and there may be all the less room for surprise at this when we reflect that we are equally powerless to explain why even the most simple chemical

union occurs. Why does oxygen unite with hydrogen to form water? why does hydrogen unite with nitrogen to form ammonia? Probably for a reason similar to that which enables colloid molecules to give rise to those much subtler combinations which form the basis of what we call 'living matter'.

It is, we think, of importance to call attention to this consideration that living matter is the result of a molecular combination, towards the occurrence of which (with suitable materials) there may be just as natural an aptitude as there is to the formation of any of the simpler combinations which are daily occurring on all sides of us. Let us look to the facts, and see how capable they are of bearing such an interpretation if we could but clear away all the misconceptions which are only too apt to warp our judgment.

Nitrate or carbonate of ammonia, free carbonic acid, and water with a few saline substances, constitute the materials which, under the influence of the modified physical forces operating in the living plant, fall into similar modes of combination, and go to increase the bulk of its living tissues. Thus out of simplest elements is living protoplasm continually being produced in the substance of every plant that grows—thus are those higher combinations, resulting in the production of living matter, continually brought about—thus is the supposed gulf between the living and the not-living continually bridged. No higher compounds are needed as starting-points for carrying on the nutrition of

plants[1]. The most simple not-living or mineral constituents coming into relation with one another in the presence of preexisting protoplasm, appear, for aught we know to the contrary, to fall at once into those subtle combinations which constitute the basis of living protoplasm[2]. The rapidity of the process mocks and defies all theoretical explanation. Here, at all events,

[1] 'M. Boussingault has demonstrated that plants in full growth always take carbon from the carbonic acid of the air, hydrogen from the water which bathes them, and frequently azote from the air. . . . The soil he used for the growth of his plants, the subjects of experiment, was a siliceous sand, which was first sifted, then kept at a red heat for some time, in order to destroy every trace of organic matter within it. It was then moistened with distilled water, and the seeds sown; after an interval of a few days, the seeds which did not germinate were removed. . . . Peas planted in a soil absolutely barren, and watered with pure water, may attain to complete maturity, passing through all the phases of their natural growth, and bearing flowers and ripe seeds. During this process, they fix a large quantity of azote, which they must derive either from the air dissolved in the water which they absorb by their roots, or from the air that surrounds their stalks and leaves.' ('Chemical and Physiolog. Balance of Organic Nature,' by Dumas and Boussingault. Lond. 1844, pp. 76–90.) Whether the nitrogen is absorbed directly from the air by the leaves, whether it passes into the plant as a constituent of the air which is dissolved in the water taken up by the roots, or whether it is derived from an infinitely small quantity of ammoniacal vapour which constantly exists in the atmosphere, is a question which cannot be considered as settled, though many probabilities point to the latter source as that whence plants derive their nitrogen.

[2] Or else it may be that rearrangements are brought about amongst the elements of the substances dissolved, and of the aqueous medium itself, resulting primarily in the formation of colloidal combinations, which secondarily (and under the continued influence of similar physical forces) are capable of permitting the occurrence of new modes of collocation resulting in the evolution of the minutest specks of living matter.

there seems to be no laborious process of synthesis—no long chain of substitution compounds—before the final product is evolved.

But the property of decomposing ammonia and of feeding upon elementary mineral substances is by no means confined to the higher plants. The same power is possessed by *Conferva vulgaris* and other low algæ, as was demonstrated by M. Bineau nearly twenty years ago[1]; whilst nearly ten years afterwards it was ascertained by M. Pasteur that some of the lowest kinds of fungi, the *Mucedineæ*, were capable of growing and multiplying in a solution of sugar and tartrate of ammonia, to which a trace of some phosphate had been added. Referring to Pasteur's observations, Baron Liebig says:—'It is astonishing that this discovery has not attracted more attention in regard to a special point, for it comprises a fact of very great significance

[1] His experiments were made with *Conferva vulgaris* and *Hydrodictyon pentagonale*. M. Bineau says:—'Des quantités jugées à l'œil égales entre elles de chacune des deux espèces d'Algues mentionnées furent enfermées dans des flacons à l'émeri bien bouchés d'un peu plus d'un demi-litre, avec 250 centimètres cubes d'eau, contenant 12 millionièmes d'ammoniaque, ajoutée à l'état de chlorhydrate et une quantité un peu moindre d'azotate de chaux. Les flacons furent ensuite exposés, les uns sur une fenêtre où ils recevaient les rayons du soleil les autres dans le voisinage, mais dans l'obscurité. . . . Après dix jours, le liquide de chaque flacon fut filtré et soumis à un essai ammoniamétrique. . . . On a trouvé que l'*Hydrodictyon* avait fait disparaitre au soleil presque les trois quarts de l'ammoniaque, et le *Conferva vulgaris* près de la moitié. A l'obscurité l'absorption de l'ammoniaque fut environ moitié moindre. . . . Dans aucun des liquides des flacons il ne resta la moindre trace appréciable d'azote.' ('Mém. de l'Acad. des Sciences de Lyon,' t. iii. 1853.)

for physiology, viz., the formation of albuminates in plants, respecting which we are in possession of scarcely anything beyond conjecture; hitherto this has been regarded as one of the greatest mysteries of organic nature. . . . . If yeast cells, placed in a mixture of ammonia, tartaric acid, sugar, and phosphate, could propagate and multiply, it is evident that an albuminate must have been formed from the elements of this mixture, since one of the chief constituents of the yeast fungus is an albumenoid substance[1].'

All that is here said by Liebig becomes even still more striking after my own observations, as to the freedom with which *Bacteria* and *Torulæ* multiply not only in solutions of ammonic tartrate to which a phosphate has been added, but also in solutions of tartrate of ammonia alone. The fact that this occurs shows that these simple saline substances not only contain the elements necessary for the formation of living matter, but that the passage must be comparatively easy from the saline mode of collocation of the elements

[1] Although quite willing to believe that this may take place, Liebig contends that Pasteur has not proved that it does occur. Some of Liebig's objections are, however, we think, based upon possible misconceptions. Actual beer-yeast contains sulphur as a constituent, an element which was not known to exist in Pasteur's mixture. It seems quite possible, however, that *Torulæ* closely resembling beer-yeast in appearance may exist, into whose composition sulphur does not enter. The freedom with which *Bacteria* and *Torulæ* develop in a simple solution of tartrate of ammonia in distilled water make it doubtful whether the presence even of phosphorus is absolutely necessary for the formation of the simplest kinds of protoplasm.

into that by which they are converted into living protoplasm. Nay, more, seeing that the multiplication of living things takes place with so much more energy and rapidity in a solution of ammonic tartrate than it does in one of the oxalate, the acetate, or even the carbonate, it seems to show that the ammonic tartrate state of combination is an especially favourable platform for the initiation of these new and more complex modes of combination [1].

Again, then, it may be argued that the production of living matter from such simple not-living constituents could not take place unless there were a great natural tendency for the molecules of certain compounds to fall into the more complex modes of combination which exist in living matter.

If in answer to this it is urged that such mysterious combinations can only occur in connection with, and under the immediate influence of, pre-existing living matter, the reader will now be in a position to estimate the real value of the reply. We have shown how overwhelming is the evidence in favour of the *de novo* evolution of living matter even under the influence of conditions which might be deemed little favourable for the occurrence of such a process. If, then, such combinations can occur after the materials have been exposed to the influence of very high temperatures within hermetically-sealed flasks, how much more likely are they to take place when unaltered organic solutions are freely

[1] See *Appendix C*, pp. xlvi-xlviii.

exposed to various physical agencies, which play upon them in the world without, and how probable does it become that living things are continually arising *de novo*, on account of the 'spontaneous' occurrence of such combinations wherever organic matter exists in solution. It is only by denying such possibilities—now almost converted into certainties—that biologists can reject the notion of living matter being formed by virtue of chemical combinations which are naturally prone to occur when heat and other physical forces act upon suitable materials—just as chlorine is prone to unite with hydrogen under the influence of light, or just as cyanimide has a natural tendency to unite with glycocol, when both coexist in aqueous solution, so as to form glycocyamine.

But few can bring themselves to look at the facts in an unbiassed manner. Refuge is unconsciously taken in the last stronghold of vitalism: powerfully influenced by an analogical argument in support of their belief in the continuity of life, certain biologists in the present day would endow pre-existing protoplasm with marvellous and unique powers, at the same time that they deny the existence of any special vital force. They have not yet fairly cast off the old vitalistic theories which they profess to repudiate. They shut their eyes against, or will not be convinced by, all the evidence which speaks loudly for the 'spontaneous' occurrence of the changes which give birth to living matter, and consequently they still proclaim a belief in their

favourite assumption, as to its sole origin under the influence of pre-existing protoplasm. Thus alone are they enabled to deny what others believe to be the proper interpretation of known facts; thus will they reject the conclusion that there is a natural tendency amongst certain kinds of molecules to fall into combinations and rearrangements which terminate in the formation of 'living' matter.

So much may be said concerning the origin of specks of living matter in the flasks containing organic compounds. And with reference to those in which a small portion of organic matter—cheese, for instance—has been added to a saline solution containing the elements necessary for the nutrition of the simplest living things, the origin of those that have been found may perhaps have been due to rearrangements which took place amongst the elements of the added organic matter; or else the molecular changes which it initiates may have sufficed to induce life-giving combinations amongst the disturbed elements of the saline substances themselves[1]. The living specks thus initiated would subsequently grow and multiply at the expense of the elements of the saline substances, just as organisms do which are purposely added to such solutions.

With reference, on the other hand, to those saline fluids in which no organic matter had been purposely added, but in which some may have existed in the form

[1] Whenever these were suitable for the initiation of such changes.

of accidental impurities, I am inclined to think that such compounds may require an admixture of some more unstable substance before living things are capable of being evolved. This more unstable substance (existing as 'dead' organic matter) may act as a ferment and may initiate changes which would not occur if the saline substance existed alone in solution [1]. Certainly, in my experiments, I have been able to find no valid evidence that living things have presented themselves without such admixture. Minute fragments of vegetable fibre of different kinds—which it is nearly impossible altogether to exclude—have almost invariably been present in the saline solutions employed. Such fragments are, indeed, constantly present within the crystals of ammonic tartrate which I have employed; whilst other evidence, previously alluded to, makes it probable that the crystals themselves contain a ferment. Thus, a solution of ammonic tartrate with some sodic phosphate when not previously heated, rapidly becomes turbid on exposure to the air or *in*

[1] Just as motion (produced by constant slight shocks) amongst the molecules of amorphous iron favours the lapse of these into crystalline modes of aggregation, so may motion amongst the particles of a saline compound tend to disturb existing modes of combination, and facilitate the assumption of new modes of combination, towards the occurrence of which there is a natural tendency. And, as Liebig says:—'All organic substances become excitors of fermentation, as soon as they pass into a state of decomposition: the changing condition once imparted, propagates itself in every organic atom, which is not itself, that is, by its own inherent energy, capable of annihilating the imparted motion by presenting an adequate resistance.' ('Letters on Chemistry,' p. 208.)

vacuo; whilst a similar solution which has been boiled for some minutes does not at all readily become turbid even when exposed to the air, although it will do so in a few hours if some living *Bacteria* be purposely added. Such facts seem to show, not only that living *Bacteria* are scarce in the air, but also—from the fact that the unboiled solution will rapidly become turbid—that the solution originally contained some ferment whose virtues were to a certain extent destroyed by the heat[1]. Destroyed, however, only to a certain extent—since the ammonic tartrate and sodic phosphate solution which will no longer become turbid from presence of *Bacteria*, will, after a long period, yield *Torulæ* or one of the simplest kinds of *Fungi*[2]. Destroyed only to a certain extent also, because the citrate of iron and ammonia solution will even yield *Bacteria* in addition to other organisms, after an exposure to a temperature of 145° C, and the solution of ammonic carbonate will also yield *Bacteria* after an exposure to a still higher temperature for a longer period. Thus the saline solutions employed have, perhaps, needed the presence of more unstable matter which might act the part of a ferment, before life-giving changes could be initiated.

However difficult it may be, at first, to imagine that living things are capable of springing up *de novo* in a solution of tartrate of ammonia and phosphate of

[1] The ferment must have been originally either in the water, in the crystals, or in both.

[2] See *Appendix* A, pp. i. and ii.

soda or in one of citrate of iron and ammonia, this difficulty is considerably mitigated if we steadfastly remember that living organisms are capable of growing and multiplying in similar solutions. This fact, that growth and multiplication can take place at the expense of the elements of the saline solution, shows that under a certain influence—that of the pre-existing living matter—the elements of the saline solution are capable of reacting in such a manner as to fall into new modes of combination, whereby they give rise to 'living' compounds. Now we must again contend that as no special or peculiar forces are at work within pre-existing organisms, the molecular movements constituting their 'life' must be determined purely by natural affinities, so that they can only exert an action which is essentially chemical upon the molecules of the matter with which they are brought into contact. If, then, under the influence of these chemical actions the molecules of the saline substances undergo a rearrangement and combination whereby they are converted into living protoplasm, we are compelled to assume the truth of what appears (as we have already said) to be on other grounds so probable, that there is a natural aptitude for the disturbed molecules of the saline substances to fall into such modes of combination. The facts revealed by our experiments compel us to believe, moreover, that the molecular movements impressed upon the saline materials by unstable, though dead, substances, are also of such a nature as to allow those

natural aptitudes of the molecules to come into play, whereby they fall into living modes of combination.

We are quite prepared to expect that, in a short time, some solution of saline substances may be discovered capable of retaining its power of passing through life-evolving changes, even after having been subjected within hermetically-sealed vessels to very high temperatures. That is to say, we believe that some day a saline solution will be found in which, without aid from co-existing organic matter, synthetic life-giving combinations may occur. In order to attain this end, a combination of substances will be needed capable of withstanding an exposure (under pressure[1]) to such high temperatures as would suffice to break up all peculiarly 'organic' compounds, and yet leave the total constituents of the sealed flask in such a condition as to enable them to lapse into living modes of combination—just as easily as the elements of ammonic tartrate and water do under the influence of a dead ferment.

These considerations are replete with interest. They insensibly lead us on to the enquiry as to whether living things can now originate upon the surface of our globe after the same manner in which alone (in accordance

[1] The higher the degree of heat, the greater does the pressure become within the flask. It must not be forgotten that under such influences alone there is the possibility of synthetic changes taking place. As before mentioned (p. 24), cyanimide (CN^2H^2) is converted into cyanuramide ($C^3N^6H^6$) at a temperature of 150°C.

with scientific teachings and the evolution hypothesis) they could have originated in those far remote ages, when what we call 'Life' first began to dawn upon the still heated surface of the earth. Before organic materials of the ordinary kind could exist, organisms must have been present to produce them. Organizable compounds of a certain kind must nevertheless have preceded organisms. And just as chemists are now able to build up a great number of so-called organic compounds in their laboratories, so it seems almost certain that some such mobile compounds may have been evolved by the agency of natural forces alone acting on the heated surface of the earth at a period anterior to the advent of living things. That mere saline substances are capable of undergoing change and rearrangements under the influence of physical forces is a well-established fact which nobody denies, and of which we have an admirable instance in the conversion of ammonic cyanate into urea. It is also certain, as Prof. Graham showed, that one and the same saline substance may exist with its molecules now in the crystalloid and now in the colloidal mode of aggregation—according to the different influences to which it has been subjected or under which it has been produced. This, for instance, is the case with silica, with the sesquioxides of chromium and iron, and with other mineral substances. Nay, more, the absence of any natural barrier between the crystalloid and the colloidal mode of aggregation may be still further seen by the

fact that even the most typical colloids are capable of undergoing that kind of isomeric molecular change which converts them into crystalloids. As one of the best instances of this we may mention the fact of the change which blood pigment undergoes. Hæmatoidin is frequently met with in the form of oblique rhombic crystals, and in addition there are other crystalline forms of albumenoid substances obtainable from blood [1]. Amongst these may be included certain tetrahedral crystals discovered by Reichert in connection with the placenta of the guinea-pig, the behaviour of which to reagents renders it certain that they were of an albumenoid or protein nature. Chlorophyll also has been observed in a crystalline state by M. Trécul [2], whilst Dr. Montgomery [3] has depicted the results of a similar change which a tube of myeline had undergone. These facts sufficiently prove that no impassable barrier exists between the crystalloid and the colloid states of matter [4]. Do we not see that simple saline

[1] See an article on 'Albuminous Crystallisation' in 'Brit. and For. Med. Chir. Rev.,' Oct. 1853.

[2] 'Comptes Rendus,' t. lxi. p. 436.

[3] 'On the Formation of so-called Cells in Animal Bodies,' 1867.

[4] In a paper recently read before the Royal Society (Proceedings, vol. xix. [1871] p. 455), by Dr. Marcet, entitled, 'An Experimental Inquiry into the Constitution of the Blood and the Nutrition of Muscular Tissue,' he states, 'that a mixture of colloid phosphoric anhydride and potash can be prepared artificially by the dialysis of a solution of chloride of potassium and phosphate of sodium, and that the colloid mass thus obtained appears to retain the characters of the neutral tribasic phosphate.' Dr. Marcet finds, moreover, 'that blood contains phosphoric anhydride and iron in a perfect colloid state, or

substances may pass into the colloidal condition, and that even typical colloids may assume a crystalloid mode of aggregation? It surely is not difficult to imagine, therefore, that molecular rearrangements may take place amongst the constituents of ammoniacal salts of greater complexity, whereby a more complex colloid may be produced—one which may differ in no essential respect from the simplest forms of protein. And if such a change does take place, it would be only rational for us to suppose that the new-formed protein would be just as prone to undergo change as this substance generally is. If ordinary protein compounds, therefore, which have been built up in living things, are capable of going through certain life-giving changes, it would be quite natural to suppose that the differently evolved protein—that which comes into existence 'spontaneously,' or without the influence of pre existing living matter—would go through similar changes.

Wherever life-giving combinations occur, therefore, we are entitled to look upon them as actions resulting from the influence of physical forces upon material collocations whose molecular constitution is of such a nature as to render them most prone to undergo rearrangements. A series of reactions takes place

quite undiffusible when submitted to dialysis.' In summing up the results of his researches, he comes to the conclusion—'That there is a constant change, as rotation in nature, from crystalloids to colloids, and from colloids to crystalloids.'

between such material collocations and their environment, leading to further combinations, and as a result living matter appears. Such a tendency to undergo change is inherent in colloidal compounds. As Prof. Graham told us:—'Their existence is a continual metastasis. . . . The colloidal is, in fact, a dynamical state of matter, the crystalloid being the statical condition.'

CHAPTER XIII.

CRYSTALS AND ORGANISMS: CAUSES WHICH DETERMINE THEIR FORM AND STRUCTURE.

Fluidity and Solution. Molecular qualities retained. Action of Heat. Solution a State of Chemical Combination. Modes of precipitation of Saline Substances. Properties of all Bodies dependent upon Molecular Composition. Allotropism. Simple and compound Substances. Relations of Crystalloids to Colloids. Conditions favourable to Crystallization. Slowness of Union. Influence of weak Galvanic Currents. Dimorphism under different 'Conditions.' Changes in Colour as well as of Shape. Dr. Bennett's Cellular Crystals. Mr. Rainey's Calculi. Fusion of these. Structure of Starch-grains similar. Their Fusion. Albumenoid Concretions. Mere amorphous Granules. Specks of 'living' Matter. These assume Organic Forms. Products differ as Heat acts rapidly or slowly. Different origin of Crystals and Organisms. Views of Maupertuis, Burdach, Schwann, Herbert Spencer, and G. H. Lewes. Passage of not-living into 'living' Matter, in Growth of Plants. Influence of pre-existing Protoplasm determines the Quality of the new Matter. Same with pre-existing Crystalline Matter. Crystalline Polarities shown by Repair. Modifications producible by different 'Conditions.' Dimorphism of Mercuric Iodide and other Salts. Such Modifiability should be more marked in the case of 'living' Matter.

THE states of fluidity and solution are conditions to which most forms of matter may be reduced, and from which all solid forms must, in accordance with the Evolution hypothesis, have originally emerged.

Fluidity or fusion is due, for the most part, to the

dissociating agency of heat, which tends to increase the distance between the ultimate atoms and molecules of bodies [1]. The chemical affinities holding together the constituent atoms or molecules of certain compounds are, however, too feeble to withstand the dissociating influence of an intense amount of heat. As the temperature rises, the chemical affinities which bind together the dissimilar atoms into compound molecules become more and more weakened, and may be at last overcome before liquefaction takes place. Still larger is the number of compounds which are unable to endure the disruptive agency of the higher temperatures necessary to reduce them to the state of gas or vapour. In the case of those substances, moreover, which are capable of being reduced to either physical condition by the aid of heat, innumerable

[1] 'Bunsen and Hopkins have shown that substances which expand when fused have their point of fusion raised by mechanical pressure, that is to say, since mechanical force must be overcome in melting, the tendency to melt must be overcome by heat before that opposition can be overcome; and the pressure required to keep them solid at any temperature above their natural point of fusion may be looked upon as the mechanical representative of the force with which they tend to fuse at that temperature. Prof. W. Thomson has shown that, on the contrary, water, which expands in freezing, has its point of fusion lowered by pressure; that is to say, since mechanical force must be overcome by crystallizing, crystallization will not take place under increased pressure, unless the force of crystalline polarity be increased by reducing the temperature. . . . Similar principles hold true with respect to the solubility of salts in water.'—Bakerian Lecture, 'On the Direct Correlation of Mechanical and Chemical Forces,' by H. C. Sorby ('Proceed. of Royal Soc.' vol. xii. 1863, p. 540).

differences exist as to the amount of heat which is necessary for converting them into the one or the other state. In short, the particular temperatures at which different elementary or compound substances are capable of existing respectively as solids, fluids, or vapours, varies *ad infinitum*, in accordance with differences in the molecular nature and properties of the bodies themselves. Most of their distinctive chemical characters remain, however, essentially the same, in whatever physical condition the matter may at the time exist—whether that of gas, fluid, or solid [1].

When reduced to the state of solution, also, bodies lose the obvious physical characters which originally distinguished them. Their individual and separate existence has gone—their constituent molecules have parted company, and are, for the time, more intimately related to the molecules of the solvent. The solvent itself may vary much in nature, though that with

[1] The molecular relationships of liquids and their vapours has been further elucidated in a recent memoir by Prof. Tyndall, 'On the Action of Rays of high Refrangibility upon Gaseous Matter,' in which he makes the following highly interesting statements:—'1. The vaporous nitrite of amyl absorbs with such avidity the rays competent to decompose it that a very small depth of the vapour quenches the efficient rays of a powerful beam of solar or electric light. 2. The vaporous iodide of allyl, on the contrary, permits a beam to traverse it for long distances without very powerfully diminishing the chemical power of the beam. 3. The liquid nitrite of amyl, in a stratum one quarter of an inch thick, quenches all the rays which could act chemically upon its vapour. 4. The liquid iodide of allyl, on the contrary, in a stratum of four times the thickness just mentioned, does not materially diminish the power of the beam to act upon its vapour.' ('Phil. Trans.' 1870, p. 344.)

whose action we are most familiar is water. This fluid dissolves a great variety of different substances. And although the materials so dissolved lose the characteristics which distinguished them as solid aggregates—such as form, hardness, specific gravity, and other physical qualities—the actual matter is still there, in a state of molecular diffusion and with all its chemical properties comparatively unaltered. It is recoverable, also, in the form of a solid aggregate—either by the dissipation of the water by means of heat, or else by the use of reagents for which the molecules of water have a stronger affinity.

Many elementary substances and compounds that cannot be made by the agency of heat to assume the fluid form (as well as many which can be so reduced) are dissolved by immersion in water. And just as innumerable variations are met with in the behaviour of different simple and compound substances under the influence of a given degree of heat, so innumerable variations exist in the behaviour of different substances when brought into contact with water of a given temperature. Some are very soluble, some less soluble, and others quite insoluble; these differences being dependent upon the different properties of the molecules of the substances in relation to those of water. A union, which can only be termed chemical, takes place between the molecules of the substance dissolved and that of its solvent[1]; though where these molecules are complex,

[1] Speaking of the force which determines solution, Mr. Sorby says,

as with salts, they may be broken up into simpler units, which enter separately into combination with the molecules of water. The state of solution is, therefore, to be regarded as a new chemical combination—one which carries with it, like many other such combinations, marked differences in physical quality.

In such respects, therefore, the state of solution differs notably from the mere state of fluidity to which the molecules of a simple body may be reduced by the agency of heat.

But solution is a state of combination whose durability, like that of all other chemical combinations, is absolutely dependent upon the strength of the affinity existing between the molecules of the solvent and those of the substance dissolved. Solubility, accordingly, is amenable to the influence of all those causes which generally tend to affect the stability of compounds. A little diminution or a little increase of heat may render a pre-existing union no longer possible. Thus, when a hot saturated solution of alum or nitre is allowed to cool, some of the salts crystallize out of the

(loc. cit. pp. 546, 542):—'We cannot, I think, deny that the force represents some modification of chemical affinity, or is, at all events, most closely allied to it. . . . The solubility of salt in water appears to me to result from a kind of affinity which decreases in force as the amount of salt in solution increases. This affinity is opposed by the crystalline polarity of the salt; and when the two forces are equal, the solution is exactly saturated. As is well known, a change in temperature alters this equilibrium; and, according to my experiments, mechanical pressure relatively increases one or other of these opposing forces, according to the mechanical relations of the salt in dissolving.'

solution; when heat is applied to a solution of lime, some of it becomes precipitated; whilst, when either heat or cold is applied to a solution of sodic sulphate already at a temperature of 33° C, some of this salt separates from the state of solution[1]. Here, as in other cases of decomposition, the molecules of the dissolved substance and of the solvent, being themselves different, are differently affected by the influence of the same change or disturbing influence[2]. And we must suppose the amount of difference induced to be so great as to weaken or wholly destroy the affinity which had previously held them together, so that the molecules of the water under the new conditions are no longer able to hold asunder the molecules of the substance with which they were previously in combination. Or a similar effect may be brought about by the addition of a considerable quantity of a substance more soluble than that which is already in solution[3]. Thus, sodic chloride crystallizes from its aqueous solution on the

[1] Mr. Sullivan considers ('Rep. of Brit. Assoc.' 1859, p. 292) that the solubility of very many salts (like that of sodic sulphate) attains a maximum at some particular temperature, above or below which it diminishes. This temperature may frequently be above 100° C; hence the common belief that solubility *always* increases with rise of temperature, because temperatures higher than 100° C are rarely resorted to. Calcic sulphate (gypsum) is less soluble in boiling than in cold water, and is quite insoluble in water at 140° C.

[2] See vol. I. pp. 98–104.

[3] Even saturated solutions of certain substances, however, will permit a solution of some other salts without occasioning a precipitation of those originally dissolved.

addition of calcic chloride, whilst nitre does the same on the addition of alcohol. Greater solubility implies greater chemical affinity, under the influence of which, the molecules of water, leaving those of the substance first dissolved, may combine with the new molecules, whilst the old are free to aggregate in the form of a precipitated salt[1]. The case is only a little more complex where what is called 'double decomposition' takes place.

But facts of a slightly different nature must also be borne in mind. Some salts which are capable of remaining in solution together at certain temperatures, may be incapable of doing so when the solution is not maintained at a temperature within this range. In such a case, one of two things may happen: either one of the two salts originally dissolved may be precipitated, or else a 'double decomposition' may take place—leading to the deposition of one of the alternate salts,

[1] In the highly interesting memoir already referred to, Prof. Tyndall says:—'Carbonic acid is decomposed by the solar beams in the leaves of plants; but here it is in presence of a substance chlorophyll, ready, as it were, to take advantage of the loosening of the atoms by the solar rays. The present investigation has furnished numerous cases of a similar mode of action. All the vapours examined may be more or less powerfully affected in their actinic relations by the presence of a second body with which they can interact. The presence, for example, of nitric acid, or of hydrochloric acid, may either greatly intensify or greatly diminish the visible action of the light on many vapours decomposable alone or when mixed with air; while the presence of the one or the other of the same acids may provoke energetic action in substances which are wholly inactive when left to themselves.' Nitrite of amyl, nitrite of butyl, and lienol afford good examples of this mode of action, which is very similar to that referred to in the text, and which is often instrumental in aiding fermentative changes.

whilst the other acid and base still continue in a state of solution. This is an occurrence of much importance, since it tends to show that chemical affinities which may be held in abeyance at certain temperatures may, at other temperatures, assert themselves, and thus lead to the initiation of molecular combinations which result in the emergence from the solution of a new kind of solid aggregate.

We have illustrated our remarks hitherto by a reference to the behaviour of simple saline substances, though all the observations that have been made are equally applicable to chemical substances in general. It is quite immaterial whether we have to do with simple substances or with highly complex bodies: the properties of all alike are dependent upon their molecular composition and nature. *Molecular composition* is an important item even with reference to substances which are looked upon as elementary—different modes of composition or arrangement of the atoms sufficing to produce what are called 'allotropic' states. We are most familiar with these as they are presented to us in the various forms of carbon. The differences between the diamond, graphite, anthracite, and pure charcoal are most striking, and yet these are all different states of one and the same substance whose ultimate atoms are differently grouped. Oxygen[1], sulphur[2], and

[1] Ordinary oxygen, and ozone whose molecule is supposed to be represented by O_4.

[2] Sulphur crystallizes in rhombic octahedrons belonging to the

phosphorus[1], as well as arsenic, antimony and other metals, also exist in allotropic states in which they exhibit wholly different properties. It will be easily understood, therefore, that in compound substances a greater and greater possibility of molecular rearrangement arises in proportion to their atomic complexity. Gradually, in fact, this becomes the all-important character of a compound, and one to which the nature of the constituent atoms is altogether subordinate. In proof of this, one has only to refer to the multitudes of isomers with wholly different properties which are compounded of carbon, hydrogen, and oxygen, in the same relative proportions.

Although so much depends, therefore, upon the number and arrangement of the atoms in the molecule, still the properties of the molecule can be nothing more than the resultant of the properties of the different atoms—modified by their mutual influence upon

trimetric system, and also in rhombic prisms belonging to the monoclinic system. The latter have a deep yellow colour and are translucent: they always exhibit a great tendency to pass by molecular rearrangement—accompanied by an evolution of heat—into the opaque, straw-yellow, octahedral crystals.

[1] The two varieties of this substance are known by the name of Normal and Red Phosphorus. The first variety is much more poisonous than the second; it is also colourless, crystallizable in rhomboid dodecahedra, soluble in sulphide of carbon, easily oxidizable, phosphorescent, and inflammable at a low temperature. The second form is scarlet red, amorphous, much less soluble, non-phosphorescent, and only inflammable at high temperatures. Mr. Lemoine has shown that heat is the most available means for converting the one form into the other, and that the transformation is always only partial.

one another in that particular mode of collocation which belongs to, and constitutes, the molecule in question. The phenomena of allotropism, as we have previously hinted, and various other considerations, tend to show that even simple bodies—such as phosphorus, sulphur, and the metallic elements—are made up of molecules composed of similar atoms existing in a definite number and grouping in each allotropic state or separate substance. An alteration of the number or grouping of the atoms in the molecules, or of both, seems to be the only way of accounting for the wholly different properties and crystalline form of one and the same substance, such as sulphur, under the influence of different physical conditions. And thus vanishes the difference between simple or elementary, and compound bodies. They are all made up of molecules; only those of the simple substances are aggregates of similar atoms, whilst those of compound substances are aggregates of dissimilar atoms.

Different compound substances vary immensely in their degree of complexity. Some, such as ordinary acids or bases, are aggregates of simply complex molecules; others are aggregates of doubly complex molecules—that is to say, two simply complex molecules combine to form a doubly complex molecule, and, when aggregated together, these include, amongst other compounds, a very common class known as salts[1].

[1] As a general rule, it may be said that decomposition follows the reverse order. The larger molecules separate most easily; and the

Seeing that in bodies of this class a compound radicle, such as cyanogen (CN) or ammonium (NH_4), may replace one of the simple metallic elements, and that two such salts may combine together to constitute a double salt; or that the metallic element may be replaced by a more complex radicle, such as urea ($C^2 N^2 H^4 O^2$) or kreatinine ($C_8 H_7 N_3 O_2$), or even by one of the still more complex bodies known as alkaloids [1], we may be somewhat amazed at the marvellous atomic complexity which is to be attained even by crystallizable bodies known as salts.

In each case we have to do with atomic and molecular properties, and looked at in this light, the differences between what are called simple and complex substances gradually vanish. All alike have a molecular constitution, though the molecules may be simple or compound, and made up of like or unlike units.

What has been said concerning crystallizable bodies obtains also with regard to the compounds known as *colloids*. We will not recapitulate what has been already said [2] concerning these remarkable compounds; we will merely state that they are supposed to be generally characterized by the large size and complexity

constituent atoms of the simple molecules are the last to part company.

[1] The composition of narcotine, for instance, is said to be $C_{48} H_{25} NO^{14}$, and that of morphine, $C_{34} H^{19} NO_6 + 2 HO$. Both bodies have distinct crystalline forms.

[2] See vol. i. pp. 88–91.

of the molecules of which they are compounded. Prof. Graham says:—'It is difficult to avoid associating the inertness of colloids with their high equivalents, particularly where the high number appears to be attained by the repetition of a smaller number. The enquiry suggests itself whether the colloid molecule may not be constituted by the grouping together of a number of smaller crystalloid molecules, and whether the basis of colloidality may not really be this composite character of the molecule.' In all probability, two of the distinguishing characteristics of colloids, namely, their very slow rate of diffusion and their want of any tendency to assume a crystalline form, are referrible to this large size and complexity of the molecules of which they are compounded. No hard and fast line, however, separates the colloids from the crystalloids. Although multitudes of bodies exist which may be easily placed in one or the other class, multitudes of others are to be met with having properties of an altogether intermediate character. Nay, even the most typical colloids may undergo a rearrangement of their elements, whereby they are converted into crystalloids[1]. Nothing could show more plainly than this that the difference between a crystalloid and a colloid is merely one of degree, and that the properties of colloids are different merely by reason of the more complex molecular arrangement

[1] See p. 39.

which prevails—an arrangement, however, which, by the mere influence of physical conditions, the molecules of certain crystalloids are known 'spontaneously' to assume. A further consequence directly flowing from this superior complexity of the colloid molecule, is, as we have previously endeavoured to show, the greater instability which characterizes them as a class. Very slight changes in the conditions or influences to which the colloid is exposed lead to changes in its constitution—owing to the ease with which a re-arrangement is brought about amongst its constituent atoms or elementary molecules. Its very existence, as Prof. Graham pointed out, is one of 'continual metastasis,' and 'may be compared in this respect to water while existing liquid at a temperature under its usual freezing point, or to a supersaturated saline solution.'

Colloids, like crystalloids, are soluble—sometimes largely so—though generally 'they are held in solution by a most feeble force.' The feeble force with which, when in the state of solution, their molecules are combined with those of water, is another peculiarity eminently favourable to the occurrence of rearrangements or recompositions amongst their molecules. Thus when in a state of solution colloids are most favourably situated, in all respects, for undergoing whatever changes the incidence of physical forces is capable of effecting.

It will be well now to enquire a little more fully

into the nature and respective characters of the solid aggregates, which, under the influence of different conditions, may be made to emerge from crystallizable and colloidal solutions respectively.

Although saline materials so frequently aggregate into crystalline shapes when they emerge from the state of solution [1], still, in many cases, the assumption or not of such a form is entirely dependent upon the conditions under which the separation takes place.

Many substances which, in the chemist's laboratory, are only seen in the form of insoluble precipitates made up of amorphous granules, could have been procured in a crystalline condition if the same decomposition which had given rise to the amorphous precipitate had been allowed to take place more slowly.

[1] The conditions under which crystallization occurs are thus given in Watts's *Dictionary of Chemistry*:—'To enable a body to assume the crystalline state, its particles must possess a certain freedom of motion; hence the fluid state is for the most part an essential preliminary to crystallization. Sometimes, indeed, an amorphous solid—that is to say, one which has no definite structure, either crystalline or organized—passes spontaneously into the crystalline state without previous liquefaction. . . . . . . But, generally speaking, it is in the passage of a body from the liquid or gaseous to the solid state that the regular and symmetrical arrangement of the molecules takes place which constitutes crystallization. The vapours of many substances when they come in contact with cold surfaces pass at once to the state of crystalline solids, e.g. sulphur, iodine, benzoic acid, arsenious acid, camphor, &c. It is, however, in the transition from the liquid to the solid state that crystallization most frequently takes place. If the body has been brought into the liquid state by the action of heat alone, it may be made to crystallize by cooling, e.g. bismuth, sulphur.'

If, instead of pouring a certain amount of a solution of potassic sulphate into one of baric chloride, we allow the mixture to take place gradually by means of dialysis, then crystals of calcic sulphate are formed rather than an amorphous precipitate. It has been ascertained by M. Frémy[1] that insoluble compounds generally, which appear in the laboratory as a result of double decomposition in the form of amorphous precipitates, can almost invariably be obtained in a crystalline condition when the chemical reaction is allowed to take place very slowly. This may be brought about by making the saline solutions mix after osmosis—either through membranes, wooden vessels, or porous porcelain. By one or other of these methods, he obtained many very insoluble salts in the crystalline condition—such as the sulphates of baryta, strontia, and lead, the carbonates of baryta and lead, oxalate of lime, chromate of baryta, and several sulphides[2]. These facts are highly interesting because a

[1] 'Compt. Rend.' t. lxiii. p. 714.

[2] We find in Watts's *Dictionary* the following statement, which has an explanatory bearing upon what has been above stated:—'The more slowly the liquified body is brought back to the solid state, and the more the liquid is kept at rest, the smaller the number and the greater the size and regularity of the crystals; but if the solvent be cooled or separated quickly the crystals are numerous, but small and ill-defined. In the former case, the particles of the solidifying body have time to unite themselves regularly with those which separate first from the fluid and form nuclei of crystallization; if, on the contrary, the crystallization takes place rapidly, a *great number of particles solidify at the same time*, each forming a nucleus to which other portions attach themselves, and

consideration of the circumstances under which such substances are found naturally, as crystalline minerals, makes it probable that they have also resulted from double decompositions brought about with great slowness.

It was, moreover, ascertained by M. Bécquerel and Mr. Robert Were Fox, that many crystalline minerals which had not previously been procured artificially, were to be obtained by the long-continued action of weak galvanic currents upon solutions containing the necessary ingredients. These investigations were afterwards taken up by Mr. Crosse, who succeeded in procuring a long list of crystallized minerals similar to those which had hitherto been known to exist only in mineral veins and other situations [1].

Variation in the 'conditions' under which the crystallization of any particular substance occurs, often gives rise to the most marked variation in its crystalline form [2]. Thus, referring to the article 'Dimorphism' in Watts's 'Dictionary of Chemistry,' we find the following statements :—' Many substances, both simple and compound, crystallize in forms which belong to two or three different systems of crystallization, or which, even if

thus we obtain a number of crystals irregularly formed and interlacing each other in all directions.' The transition between small, ill-formed crystals and mere amorphous granules, is easily to be accounted for by a still greater rapidity of separation.

[1] For a brief account of these experiments, see 'Report of British Association' for 1836.

[2] See Fig. 44.

they belong to the same system, yet exhibit such differences in their corresponding angles as to render it quite impossible to reduce them to the same form: this was first shown by Mitscherlich, in 1823 (Ann. Ch. Phys. [2] xxiv. 264). Such bodies are said to be dimorphous and trimorphous. The difference of crystalline form which they exhibit is associated with difference of specific gravity, hardness, colour, and other properties. Whether a body shall crystallize in one system or another seems to depend chiefly upon temperature . . . . . Sometimes the form of the crystal varies according to the solvent from which it separates: thus arsenious anhydride crystallizes from water or hydrochloric acid in regular octahedrons, but from alkaline solutions in trimetric prisms.' Taking some other specific instances, we find that—'a hot solution of saltpetre yields, when slightly cooled, nothing but prismatic crystals, but at 10°C prismatic and rhombohedral crystals appear together; if alcohol be added, the latter are formed most abundantly; the addition of potash, nitric acid, or nitrate of sodium produces no alteration.' Again, the modifying influence of temperature is shown by the fact that,—'If a solution of carbonate of calcium in water containing carbonic acid be left to evaporate at the ordinary temperature, nothing is obtained but calc spar, in microscopical, and, for the most part, truncated primitive rhombohedrons; if, on the contrary, the solution be evaporated over the water-bath, arragonite is obtained in small six-sided

prisms—mixed with a few crystals of calc-spar, because the temperature of the solution is lower at first than it afterwards becomes.' In other examples we may find not only a striking difference in physical form, but also a notable change in colour, occasioned by the new molecular rearrangements which the change of temperature seems to necessitate:—'Protoxide of lead crystallizes after fusion, as well as from a saturated solution in hot concentrated caustic potash, in yellow rhombic octahedrons. If, however, the solution is not fully saturated with oxide of lead, so that crystallization does not take place till after complete cooling, red crystalline scales are deposited on the yellow rhombic octahedrons just formed: if the red crystals are heated, they turn yellow in cooling, in consequence of passing into the first form.'

But in addition to these variations in which a crystalline form of one kind or another is always present, we may occasionally find that in saline solutions, solid products make their appearance whose outlines are rounded instead of being bounded by right lines and angles. Thus, Dr. Hughes Bennett[1] has figured a peculiar pellicle that forms on the surface of lime-water, in which the crystalline material very closely resembles a layer of tesselated epithelial cells. And lately I have seen a somewhat similar crystalline pellicle make its appearance on the surface of a solution (*in vacuo*) containing small quantities of ammonic sulphate and

[1] 'Lancet,' 1863, vol. i. p. 3.

potassic bichromate[1]. The cell-like compartments were here much smaller than those of the substance described

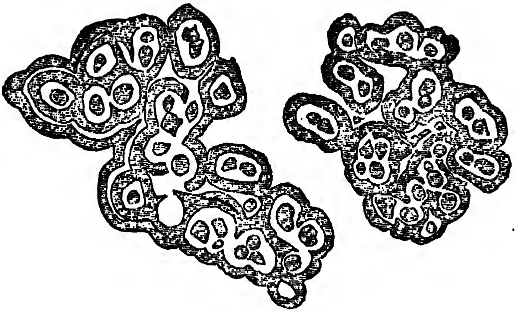


FIG. 39.

Cellular Forms of Crystalline Matter, from a solution of Ammonic Sulphate with Potassic Bichromate. (× 800.)

by Dr. Bennett, and each contained one or two unclear-looking particles. The whole mass polarized light in the most beautiful manner, and sank rapidly in water when its upper surface was wetted.

In addition to these modifications of crystalline form under different conditions, there are still others whose nature has been elucidated by Mr. Rainey. He has shown that when carbonate of lime is slowly precipitated in viscid solutions of gum, albumen, or even glycerine, the molecules of the nascent carbonate unite with portions of the viscid ingredient, and then — instead of arranging themselves into either octahedral or hexagonal crystals—the combined parti-

[1] See vol. i. p. 451.

cles assume the form of calculi, with distinct concentric layers. These experiments have already been alluded to [1], and I will now only add a few additional particulars, which have a most important bearing upon our present subject. Mr. Rainey says:—'The mechanical conditions required to act in conjunction with the chemical means are, the presence of such a quantity of the viscid material in each solution as will be sufficient to make the two solutions, when mixed together, of about the same density as that of the nascent carbonate of lime, and a state of perfect rest of the fluid in which the decomposition is going on; so that the newly-formed compound may be interfered with as little as possible in its subsidence to the sides and bottom of the vessel. This will require two or three weeks or longer, according to the size and completeness of the calculi [2].' The early forms assumed by this globular carbonate of lime [3]

[1] See vol. i. pp. 302–304.

[2] Just as the process of crystallization can be induced when it would not otherwise occur, by the influence of electricity, so Mr. Bridgman, of Norwich, has ascertained that under its influence the formation of these calculi may be materially hastened. By availing himself of the influence of a weak galvanic current he has succeeded 'not only in producing them in a very much shorter period of time, but also in obtaining a membranous matrix out of albumen, having within it these deposits in a definite layer, coalescing and aggregating together, and closely approaching the appearance presented at the edge of a natural bone in its early stage of formation.' ('Transact. of Odont. Soc.' vol. iii. p. 410.)

[3] Mr. Rainey found that 'muriate of baryta and muriate of strontia when treated in the same manner as the muriate of lime, furnish each a globular carbonate, the spherical form of the latter being particularly

are strikingly like the forms exhibited by the simplest organisms. They seem to increase in size, however, by a very remarkable process of 'coalescence,' all the

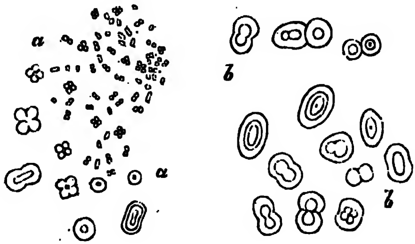


FIG. 40.

Globular Carbonate of Lime. (Rainey.)
a. Earliest forms assumed.
b. Larger globules showing different stages of coalescence. (× 450.)

steps of which have been fully described by Mr. Rainey. He gives an account of the process by which lamination takes place, and also describes the mode in which two or three of these calculi, coming into contact, will gradually fuse by a process of molecular rearrangement

perfect and beautiful. But muriate of magnesia when decomposed in the same manner and under precisely the same conditions, does not furnish globules, but crystals of carbonate of magnesia, evincing no tendency to become globular.' These various bodies have different atomic weights, and this, doubtless, has much to do with the difference of result. The atomic weights of the four are as follows:—Barium 137, strontium 87·5, calcium 40, and magnesium 24. That magnesium is too light to enter well into combination with the gum is rendered all the more probable by the fact that the effects with strontia are even better than those with lime.

into a single larger calculus. The change is a most remarkable one, during which there is brought about 'the perfect coalescence into one, of two or more globules of carbonate of lime as much as $\frac{1}{125}''$ of an inch in diameter, perfectly transparent, of a hardness nearly equal to that of glass . . . the incorporation of these globules being so complete, that the resulting one has the same spherical form, the same degree of trans-

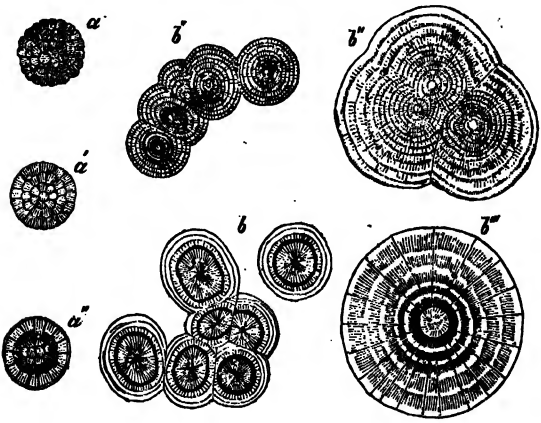


FIG. 41.

Globular Carbonate of Lime in later stages—formation of Calculi by 'Molecular Coalescence.' (Rainey.)

a a' a'' Mulberry-like bodies due to the aggregation of several small globules. These gradually undergo change from circumference to centre. The external globules first coalesce into an amorphous granular layer, which gradually becomes more transparent, and similar changes extend inwards.

b b' b'' b''' Further stages of aggregation and molecular coalescence.

parency, and the same hardness and structure as the component ones.' And yet the fact that these effects are brought about solely 'by the mutual attraction of the two globules,' must show, quite plainly, that the molecules of such structures have an extraordinary mobility and capability of rearrangement which may dimly remind us of the molecular mobility and organizing tendencies of living matter.

It is a fact of considerable interest, moreover, that the shapes of such bodies should so closely resemble those of primordial living things. The shapes of the former are undoubtedly determined by the mere physical properties of the molecules of which they are composed, which, owing to the combination of the saline matter with some of the viscid material, are probably large and complex[1]. And, therefore, we can only suppose that

[1] Mr. Rainey says:—'If the density of the alkaline solution exceed much the degree mentioned in the formula, and if that of the simple solution of gum is not equal to the degree there specified, the alkali diffusing itself through the simple solution of gum more rapidly than the gum contained in the lower solution, a larger quantity of carbonate will be formed than there will be gum to combine with it in the proportion necessary to form the globular carbonate, and consequently the carbonate of lime formed in the upper part of the bottle will be deficient in gum, and therefore it will be crystalline and not globular . . . . . . Hence, in the case just specified, the uppermost part of the deposit will exhibit perfect crystals, that immediately beneath it crystals beginning to have their angles rounded off, and the examination thus continued successively upon still lower portions will show the gradual passage of imperfectly rectilinear figures into forms perfectly spherical. . . . . . In the former case the molecules of carbonate of lime are uncombined, and, therefore, in its crystalline state it may be regarded as pure; in the latter, the carbonate of lime is combined with the viscid substance, as can be shown

the shapes of organisms (which are also compounded of complex colloidal molecules) are in all probability solely due to the properties of their molecules, as operated upon by surrounding influences. There is, therefore, a double approximate similarity of these aggregates to organisms: first, on account of their molecular mobility, and secondly, by reason of the forms which they assume on emerging from the state of solution. And both these characteristics are probably referrible to the molecular complexity of their component units.

If we now turn our attention to the solid aggregates which may emerge from solutions of colloidal matter, we shall find that they also are extremely variable in nature, according to the 'conditions' under which they are formed.

The starch-grains gradually deposited within the cells of certain plants, present a structure which, in many respects, closely resembles the calculi we have just been describing. They, too, are produced slowly, and apparently by a process of deposition within the tissue of the plant; they frequently coalesce; they exhibit a laminated structure; and they polarize in a very characteristic manner. The transition from the calculi previously described to starch-grains is, in fact, most easy and natural, since Mr. Rainey has ascertained that a certain amount of mineral matter, in the form

by chemical analysis, and therefore, in its globular form it is obviously an impure carbonate—a compound of this substance and gum or albumen.' (Loc. cit., pp. 35 and 31.)

of silica and phosphates, always enters into the composition of the latter. In the calculi there is much mineral matter and a smaller quantity of colloidal material; whilst in starch-grains there is a large proportion

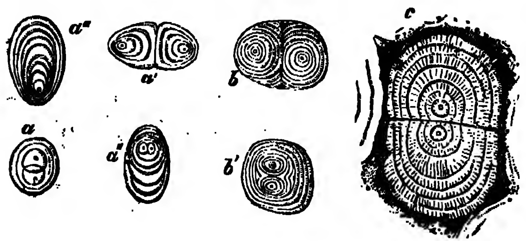


FIG. 42.

Different kinds of Starch-granules contrasted with Globular Carbonate of Lime. (Rainey.)

a–a'''. Ordinary forms of potatoe-starch.
b, b'. Compound granules of 'tous les mois' starch.
c. Two carbonate of lime 'calculi' coalescing, from the calcifying shell of an oyster.

of colloidal material and a small proportion of mineral matter.

The mode of appearance and the character of the starch-grains which are to be found in the cellular tissue, are best studied by making thin sections of any growing plants in which starch is usually formed in large quantities. Very young potatoe-tubers answer well for this purpose. When submitted to microscopical examination, Mr. Rainey says[1]:—'In such sections, in this and the majority of plants, the starch-cells in the

[1] See 'Jrnl. Microsc. Science,' 1860, p. 2.

vicinity of the ramifications of the vessels will be seen to contain very small spherules of starch, many of them too minute to be accurately measured; yet, notwithstanding their minuteness, their figure is well defined, and they are made black or blue by iodine, proving that they are as much starch as the larger globules, and differing from them in nothing but size. These spherules may be either free in their starch-cells or conglomerated and joined together in pairs or threes, producing dumb-bell or somewhat triangular forms. Sometimes they are found with shreds of membrane, and at others are invested more or less by an utricle. In the starch-cells more remote the granules are larger and fewer, so that their increase in size is attended with a diminution in number, showing most clearly that the largest are the product of the union of those of an inferior size. Indeed, the number of granules of a small size is such in some of the starch-cells that it would be impossible that they all could become developed into large granules without the space containing them undergoing a most inordinate increase in size, which is not the fact; the spaces in which the middle-sized granules are lodged being about the same size as those containing the largest granules. But the chief evidence in support of this conclusion must be obtained from the microscopic examination of all the various forms of starch, beginning with that which is merely granular and going up to that which is most perfect. Such an examination will show that there are exactly the same class of appear-

ances to be found in starch, indicative of a coalescence of its particles, as are presented by the several forms of carbonate of lime, whether prepared artificially or occurring in organized tissues.'

Other substances, such as leucine, found in animal fluids, appear as minute hyaline bodies which exhibit concentric markings—although they yield no colour reactions with the polariscope. I have frequently seen, in drops of blood taken from persons suffering from various diseases, bodies almost similar in appearance and about $\frac{1}{1000}''$ in diameter, which probably had an albuminoid constitution. In other cases irregular aggregations of various sizes, composed of rounded bodies presenting no concentric markings, have been met with; and these I have been in the habit of regarding as insoluble modifications of an albuminoid substance, which had probably been reduced to this state by reason of some molecular rearrangements that had taken place in the dissolved materials of the fluid from which they have been derived. A change might easily occur whereby the previously soluble compound becomes no longer soluble; and then, as it appears, it may separate from the blood and grow in the form of rounded masses. And these forms are just as much the expression of its resultant molecular attractions as the intersecting fibres of fibrine which separate[1]

[1] The process may be watched in a drop of the blood beneath the microscope; and all the more easily if the specimen has been taken from the finger of a person suffering from rheumatic fever.

from coagulating blood represent morphological states necessitated by its molecular affinities.

Occasionally, too, when organic infusions are exposed to conditions unfavourable for the formation of organisms (more especially when their virtues have been, in great part, exhausted by a previous abundant growth), this kind of pseudo-crystallization occurs, and peculiar

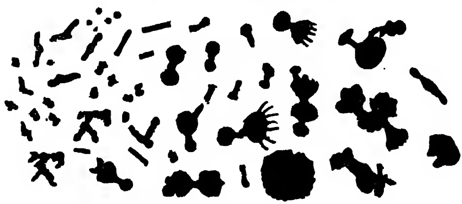


FIG. 43.

Peculiar Forms assumed by Albuminoid Concretions from an old Hay Infusion. (× 800.)

bodies are produced, which seem to grow after the fashion of crystals into all kinds of odd shapes. A great similarity of form, however, often obtains between the concretions which occur in the same fluid. Such aggregates are in fact in every way analogous to crystals, and their differences of form are probably just as referrible to differences of molecular composition [1].

It has been already pointed out that the products obtainable from certain saline solutions differ according

[1] We may here draw the reader's attention to the different shapes which are assumed by the granular particles entering into the composition of the pellicles or deposits found after a time in certain saline solutions. They were curiously branched and knobbed in Experiment *w*.

to the rate at which they emerge from the state of solution. Molecules which come together suddenly and tumultuously aggregate into mere formless granules, whilst those which come together more slowly are enabled to collocate into that geometrical form in which their molecules exist in a state of polar equilibrium. The crystalline is, undoubtedly, a higher mode of aggregation than the amorphous, and it is the highest form which it is possible for the molecules of a crystalloid to assume. But when we turn to the different kinds of colloidal matter, we shall find, as might be expected from a consideration of its superior molecular complexity, that a still more marked difference exists between the various solid aggregates which, under the influence of different conditions, can be made to emerge from solutions containing such matter.

When many albuminous fluids are heated to 140°–212° F, it causes an isomeric modification of the protein substances and their precipitation throughout the uid in the form of minute particles[1]. These are tolerably similar in size and form to those which appear when a saline substance is precipitated in the condition of an insoluble powder, owing to the rapid and simultaneous union of constituents whose affinity for one

[1] This is one of the best means of seeing Brownian movements of the most typical kind, especially if a drop of the fluid be examined whilst still warm. Heavier saline granules no larger in size may not exhibit such movements at all, or else only very imperfectly. The albuminoid molecules also cease to vibrate in syrup, glycerine, or fluids of similar consistence.

another is much stronger than for the molecules of the solvent. But the slow and long-continued action of a more moderate amount of heat, will lead to those higher kinds of collocation that are possible for such more complex molecules, and by which specks of what we call 'living' matter make their appearance throughout the fluid. These speedily grow into primordial organisms known as *Bacteria*, and—more especially if the separation has taken place still more slowly—into *Torulæ* or other kinds of *Fungus*-germs. We must consider such well-known organisms to be just as much immediate products derivable from colloidal matter, as crystals are the results of those modes of aggregation which are habitual and necessary among simpler molecules [1].

When we consider, moreover, that *heat* is consumed in the building up of colloidal matter, and in the growth (and therefore probably in the genesis) of organisms, whilst, on the other hand, heat is emitted or given out when crystals form, we may get some dim indications as to why the latter are stable or statical

[1] We have already mentioned the fact shown by Crosse, that electricity has a most marked influence in determining the formation of crystals. It has been seen also, from the observations of Bridgman, that electricity hastens the formation of such artificial calculi, as were described by Rainey. From the fact of the great proneness of organic substances to putrefy or ferment during or before the advent of a thunder-storm, and in view of the definite observations recorded at vol. i. p. 288, it would seem highly probable that electrical influence may also favour the formation of organisms and the evolution of living matter.

aggregates, and why the former are unstable or dynamical aggregates. The molecules in the crystal are delivered over to the action of their natural affinities, which are perhaps few and simple; whilst in the colloidal and living aggregates more and more potential heat or motion is retained, so as to constitute an inherent condition so favourable to molecular rearrangements, that they are induced by the slightest external influences.

The highest product formable from the one kind of matter is known as a *crystal*, whilst the highest product formable from the other is called an *organism*. We name the process by which the one arises 'crystallization,' and that by which the other appears 'archebiosis' —with the understanding that, in the latter case, a material combination has been initiated possessing such a set of qualities as we are accustomed to designate by the word 'life.' The crystal is resolvable into comparatively simple molecules, which have, however, themselves been produced by those combinations of acid with base which occasion their separation from the state of solution; whilst the organism is made up of highly complex constituent molecules, which have been derived from synthetic changes amongst colloidal molecules preceding the final union which causes them to separate from the state of solution. Just as the origin of the crystal is due to the operation of molecular affinities, under the influence of the 'conditions' which are operative in its medium, so is the origin of the

organism referrible to molecular affinities, of a more complex order, under similar influences. Nay, further, just as the form and properties of the crystal are to be taken as the natural outcome of the properties of its constituent molecules under the influence of its environment, so are the form and properties of the organism to be considered as the natural outcome of the properties of its molecules, entering into combination under the influence of their environing conditions.

Views analogous to these have been more or less fully expressed by many writers. The essential similarity in the laws regulating crystalline and organic forms was even suggested by Maupertuis in 1744[1]. Crystals and organisms were spoken of by Burdach[2] as statical and dynamical aggregates respectively. We have seen, moreover, that the formation of organisms

[1] Milne-Edwards says ('Physiologie et Anatom. Comp.' t. viii. p. 247): 'Maupertuis dont la célébrité est due surtout au voyage qu'il fit en Laponie avec Clusant et quelques autres savants pour vérifier les idées de Newton touchant l'aplatisement de la terre aux poles combattait fortement la théorie de la préexistence et de l'emboitement des germes. Il crut pouvoir expliquer la formation des organismes en supposant que les molécules de la matière organisable sont douées d'une sorte d'attraction élective en vertu de laquelle ces atomes se rapprocheraient et s'uniraient dans certains rapports, de façon à donner naissance à des assemblages analogues à ceux dont ces mêmes molécules proviennent propriété qu'il comparait tantot à l'affinité chimique ou à l'attraction en vertue de laquelle les parties constitutives d'un cristal se réunissent suivant un ordre déterminé, tantot à une sorte d'instinct ou de souvenir d'un état antérieur. Les premiers écrits de Maupertuis sur ce sujet parurent peu d'années avant ceux de Buffon.' (Œuvres, t. ii. p. 3, 1744.)

[2] See vol. i. p. 298, note 2.

was, in 1836, definitely compared by Schwann to the formation of crystals. Cells, which were then believed to be the types of all rudimentary organisms, were thought by him to owe their form to a process essentially similar to crystallization; the characteristic shapes being due, in the case of cells, to a peculiarity in the nature of the substance of which they were composed. As Schwann expresses it:—'The formation of the elementary shapes of organisms is but a crystallization of a substance capable of imbibition. The organism is but an aggregate of such imbibing crystals[1].' But as may have been gathered from statements already made, Mr. Herbert Spencer has, within the last few years, much more fully worked out the doctrine that the structures and shapes of organisms are the results of the 'polarities' of their constituent organic units, under the continually modifying influence of external conditions[2]. To his full and admirable treatment of

[1] A doctrine somewhat similar to Schwann's has been more recently advocated by Dr. Montgomery. He sums up a memoir published in 1867, 'On the Formation of so-called Cells in Animal Bodies,' with these words:—'The experiments which I have communicated go a good way to show that a plastic imbibing material driven into individual shapes by a crystallizing influence is the cause of "cell" formation.'

[2] See more especially the Appendix to his 'Principles of Biology.' In the work itself he appeared to lay too much stress upon 'inherent tendencies,' and this has given rise to adverse criticisms. In the Appendix, Mr. Spencer admits that he did not 'adequately explain' that 'the proclivity of units of each order toward the specific arrangement seen in the organism they form, is not to be understood as resulting

the subject we shall subsequently allude more in detail. We must, however, now call attention to a very important essay by Mr. G. H. Lewes [1], in which he prominently develops those aspects of the doctrine which previously had not been sufficiently elucidated. He dwells upon the extreme importance of variation or similarity in external conditions in inducing living things (in which 'we observe such a community of elementary substance') to vary or resemble one another in their organic forms. In one passage he says [2]:—'Although observation reveals that the bond of kinship does really unite many divergent forms, and the principle of Descent with Natural Selection will account for many of the resemblances and differences, there is at present no warrant for assuming that all resemblances and differences are due to this one cause, but, on the contrary, we are justified in assuming a deeper principle which may be thus formulated: All the complex organisms are evolved from organisms less complex, as these were evolved from simpler forms: the link which unites all organisms is not always the common bond of heritage, but the *uniformity of organic laws acting under uniform conditions*. . . . . It is therefore consistent with the hypothesis of evolution to admit a variety of origins or starting-points.' In this paper the immense

from their own structures and actions only, but as the product of these and the environing forces to which they are exposed.'

[1] 'Darwin's Hypotheses,' in Fortnightly Review, 1868.

[2] Loc. cit., p. 370.

importance of the influence of external conditions as compared with that attributable to the inherent aptitudes of living protoplasm has been dwelt upon perhaps more strongly than by any previous writer, and has led Mr. Lewes to announce a most important modification of the Darwinian hypothesis. Before I had read Mr. Lewes's essay, though several months after it had been published, the results of my own experiments had driven me to adopt a similar notion. When once it had been proved that living matter could come into being *de novo*, a belief in the truth of such doctrines was a logical necessity, as further explanations will suffice to show.

Crystals can be traced back only as far as their first emergence in a speck-like form from the fluid in which they arise; and similarly, some organisms can be traced back to the minutest visible specks which appear in a filtered infusion of organic matter. Concerning the existence of *invisible* germs of organisms we have no knowledge—we know no more of them, and have little more right to assume their universal diffusion than we have to calcu'ate upon the universal diffusion of invisible crystalline germs. We have seen, moreover, how the experimental evidence completely upsets the mere hypothesis that *all* living things which now appear have been derived from pre-existing living things; and on the other hand, we know that an overwhelming amount of evidence can be brought forward to show that the first particles of organisms may be

produced under the influence of physical forces alone, just as it is admitted that the first particles of a crystal are formed by 'spontaneous' combination. Just as all subsequent portions of the crystal form in obedience to the same molecular properties and physical forces as lead to the collocation of the first particles, so may the first particles of an organism combine by virtue of the same chemical and physical influences as those which subsequently determine its increase[1].

The plant takes not-living mineral ingredients from earth and air, and under the influence of solar light and heat, these simple materials assume within its tissues those higher modes of combination which are necessary in order that they may be converted into 'living' matter. The forces at work *in* and *upon* the plant are supposed to be nothing more than ordinary physical forces. Here, however, it is true, matter passes from the lifeless to the living state of combination under the influence of pre-existing protoplasm. No unknown and independent forces are now supposed to be at work within living tissues, and therefore we must suppose that, under the influence of the physical forces within and those without the organism, lifeless com-

[1] Living matter, like crystalline matter, is only formable by a synthesis of its elements. As Crystals have not the power of self-multiplication, they have only one mode of origin. But because Organisms have reproductive powers, the obviousness of these modes of increase has sufficed to cast doubts upon the reality of the independent origin of living units. (See vol. i. p. 473.)

pounds fall more or less immediately into such and such living combinations [1]. The influence of the pre-existing protoplasm would seem to expend itself chiefly in the building up of living matter *after its own likeness*. This, however, is a property possessed by crystals as well as organisms, and, as Mr. Lewes pointed out, such a resemblance has been alluded to by earlier writers. Mr. Lewes says [2]:—'The nourishment of various organs from a common fluid, each selecting from that fluid only those molecules that are like itself, rejecting all the rest, is very similar to the formation of various crystals in a solution of different salts, each salt separating from the solution only those molecules that are like itself. Reil long ago called attention to

[1] Here, at all events, the facts would not seem to show that there is any extraordinary difficulty to be overcome in order that matter may fall into 'vital' modes of combination. And the absolute necessity for long intervals of time during which numerous intermediate stages may be passed through, seems from this alone to be rendered somewhat improbable, even had we not an abundance of experimental evidence to bring forward in opposition to this mere theoretical supposition. We may perhaps derive some valuable hints as to the facility with which protoplasm and chlorophyll make their appearance, by reference to the changes which take place in the seed-cells of *Œdogonium*, *Palmoglæa*, and other algæ (see vol. i. p. 178). In the midst of the living tissue, fat and starch globules are gradually formed, till it seems to be almost totally converted into these products which are ordinarily deemed lifeless. But, after a time, and under the genial influence of heat, light and moisture, a molecular re-arrangement in the inverse order begins to take place. The needful elements are all there; the old combinations are disturbed, new combinations arise, and again there slowly appears the chlorophyll-containing, living plotoplasm.

[2] Loc. cit., p. 619.

this analogy. He observed that, if in a solution of nitre and sulphate of soda a crystal of nitre be dropped, all the dissolved nitre crystallizes, the sulphate remaining in solution; whereas, on reversing the experiment, a crystal of sulphate of soda is foun to crystallize all the dissolved sulphate, leaving the nitre undisturbed. In like manner, muscle selects from the blood its own materials, which are there in solution, rejecting those which the nerve will select.' And, in fact, the more we study the phenomena of nutrition, growth, and repair—wheresoever taking place—the more we may become convinced of the fact that the influence of pre-existing living matter does, in the main, show itself in this way. It may be seen by the mode in which an ulcer heals. The new skin forms, under ordinary circumstances, only at the edges of the sore in continuation with pre-existing skin; and the method recently adopted by surgeons, of transferring a small portion of epidermis to the midst of a large surface which has been denuded by a burn, is but a practical application of this physiological fact[1]. From the

[1] This admirable method of treatment was initiated by M. Reverdin, of Paris, and first practised in this country by Mr. Pollock. The latter says: 'It has appeared to me that when this process of cicatrization approaches the margin of the original ulcer, although this may have been indolent or stationary, there is a stimulus given to the latter, and a fresh process of cicatrization commences from this edge, and new tissue is formed in a direction to meet that from the transplanted portion. It has also appeared to me that the process of cicatrization is more rapid in the transplanted portion on the side nearest the edge of the original sore, when the two edges approach each other.' ('Trans. of Clinical Society,' vol. iv.)

minute transplanted mass which takes root, skin grows as from a centre, and the wound that was previously intractable rapidly heals [1].

But it may be asked, What is the cause or meaning of this tendency shown by crystals and by different kinds of living matter to mould suitable saline or organizable materials into structures similar to themselves?

In the case of crystals, only one answer can be given. There can be no reasonable doubt as to the truth of the supposition that the form of the crystal is a resultant necessity, predetermined by the molecular properties of the matter which composes it, and the sum total of conditions acting thereupon at the time of collocation. That a crystalline structure once initiated, therefore, should continue to grow in the same manner in a solution of a suitable kind, is only to be ascribed to the natural similarity of effect produced by uniform forces acting under uniform conditions. And similarly,

[1] This affords another instance illustrative of the fact that mere growth can take place under conditions amidst which development or evolution would cease. And, as Watts says (loc. cit., vol. ii. p. 115), 'crystallization is also especially facilitated by introducing into the liquid a crystal of the substances previously formed. A solution saturated at a high temperature may, under certain circumstances, be cooled down several degrees without depositing crystals; but the introduction of the crystal of a substance causes the whole to solidify instantly into a crystalline mass. This phenomenon is easily exhibited with Glauber's salt.' That this difference is only one of degree, however, is shown by the fact that crystallization will take place spontaneously if the temperature be still further lowered.

the injured form of a crystal is restored when it is placed under suitable conditions, because such a crystalline form is to be regarded as the physical expression of that mode of aggregation under which alone (within certain narrrow limits) a polar equilibrium of its molecules can exist[1]. If there is not such a connection between particular crystalline forms and certain kinds of matter under the influence of given conditions, then, what reason would there be for the uniform similarity of result which is observable? Why should different substances have definite crystalline forms? Whilst, on the other hand, if a relationship of this kind does exist, it is more easy for us to understand that the repair of a broken crystal should be effected with such undeviating regularity. The molecules of any kind of matter, when under the simultaneous influence of different forces, ultimately tend to lapse into a state of more or less stable equilibrium[2].

If further proof were needed of the truth of this view,

[1] On this subject Mr. Lewes says (loc. cit. p. 623):—'That it is the polarity of the molecules which, at each moment, determines the group those molecules will assume, is well seen in the experiments of Lavalle, mentioned by Brown (*Morpholog. Studien über die Gestaltung-Gesetze*, 1858). He showed that, if when an octohedral crystal is forming, an angle be cut away so as to produce an artificial surface, a similar surface is produced spontaneously on the corresponding angle, whereas all the other angles are sharply defined.' This cutting away of the angle of the crystal is a change which does not interfere with the essential nature of the crystalline form, so that the polar balance may be perfectly restored by the formation of another opposing flat surface.

[2] Spencer's 'First Principles,' 2nd ed. pp. 484, 495.

it would be afforded by the facts which we are now about to cite, referring to the influence of variation in 'conditions,' even upon the structure of a fully formed crystal. Such facts, however, are cited principally for the purpose of showing, more clearly than we have hitherto done[1], how very potent in some cases is the influence of varying 'conditions' in determining the nature of crystalline forms, as compared with that assignable to the inherent tendencies of the matter itself. The citation of a few of the many phenomena which are familiar enough to the chemist will serve to make plain this general principle, by showing that the whole nature of a crystal already in existence may be changed by the action of causes which seem the most trivial: a slight elevation of temperature, or even the most delicate touch, in some cases, is capable of initiating changes which spread through their entire substance, or throughout a whole aggregate of cohering crystals[2]. In the same article on 'Dimorphism' in Watts's 'Dictionary of Chemistry,' to which we have already referred, we find the following statement :—'Crystals formed at one particular temperature, and then exposed to that temperature at

[1] See p. 57.

[2] The forms displayed by cohering crystals are often most beautiful, and sometimes strikingly resemble, in their general outlines, those of shrubs or trees. We need only refer to the beautiful forms assumed by snow crystals, to the tree-like ramifications of ice-crystals on the window-pane, or to the lead and silver 'trees' which delight so many in their childhood.

which crystals of a different kind are produced, often lose their transparency, and *without alteration of external form, become changed into an aggregate of small crystals of the latter kind*: examples of this alteration of structure are afforded by sulphur, carbonate of calcium, mercuric iodide, and many other bodies.' Again:—'*Mercuric iodide* separates from solution, and likewise sublimes at a very gentle heat, in scarlet tables belonging to the dimetric system; but when sublimed at a higher temperature in sulphur-yellow, rhombic tables of the monoclinic system. The red crystals turn yellow when heated, and resume their red tint on cooling. The yellow crystals obtained by sublimation retain their colour when cooled; but, on the slightest rubbing or stirring with a pointed instrument, the part which is touched turns scarlet, and this change of colour extends with a slight motion, as if the mass were alive, throughout the whole group of crystals as far as they adhere together.' (Vol. ii. p. 332.) Then again:—'*Nitrate of potassium* usually crystallizes in the form of arragonite: but if a drop of the aqueous solution of this salt be left to evaporate on a glass plate and the crystallization observed under the microscope, it will be found that, side by side with the prismatic crystals at the edge of the drop, a number of obtuse rhombohedrons of the calcspar form are produced, just like those in which nitrate of sodium crystallizes. As the two kinds of crystals increase in size and approach one another, the rhombohedrons

become rounded off and dissolve, because they are more easily soluble than the others, while the arragonite-shaped prisms go on increasing in size. When the two kinds of crystals come into immediate contact, the rhombohedral ones instantly become turbid, acquire an uneven surface, and after a short time throw out prisms from all parts of their surfaces. Contact with foreign bodies also brings about the transformation of the rhombohedrons while they are wet. If the drops are so shallow that the liquid dries round the rhombohedrons before they are disturbed, they will remain for weeks without disintegrating, and bear gentle pressure with foreign bodies without alteration; but stronger pressure, or scratching, or the mere contact of a prismatic crystal of saltpetre, causes them to change, a delicate film proceeding, as it were, from the point of contact, and spreading itself over their surfaces; they then behave towards foreign bodies like a heap of fine dust, but retain their transparency. The rhombohedrons are also transformed, without alteration of external appearance, when heated considerably above 100° C.: they then become much harder, because the fine powder first produced bakes together into prismatic crystals.' (loc. cit. p. 333.)

These facts, together with those already cited, seem to show clearly enough, not only that the crystalline form of any crystallizable material is variable to a remarkable extent when it is allowed to crystallize under different conditions, but that, even when formed,

a crystal produced under a certain set of conditions may be compelled by its very nature, when these are changed, to undergo an entire molecular rearrangement before a polar equilibrium can be again established between the same molecules and the new influences to which they are subjected. New-born matter of all kinds ought to show, to a more or less marked extent, a similar plasticity: and if the combinations which constitute 'living matter' are more unstable than those to which we have just been referring, then the forms assumed by such sensitive matter under different conditions ought to become more and more divergent.

CHAPTER XIV.

THE FUNDAMENTAL PROPERTIES OF LIVING MATTER.

Specks of Living Matter unfold into known Forms. This also the case with Crystalline Matter. Powerful influence of 'Conditions' over Crystalline form. Transitions between Crystalline and Organic Forms. Organic Polarities shown by phenomena of Repair and Gemmation. Gradual limitation of Reproductive Faculty. Cause and Explanation of Gemmiparous Reproduction. Explanation of Reproduction of higher Organisms. Phenomena of Heredity. Nature of Germ-cells and Sperm-cells. 'Physiological units' *versus* 'Pangenesis.' Heredity the Conservative Agency. How potent, and why, in complex Organisms. Lower Organisms more and more free from influence of Heredity. Potency of 'Conditions' over Forms of lowest Organisms and Crystals. How to account for Origin of present lowest Organisms. Either from Archaic Ancestors or else comparatively New Products. Probability, and proof, of latter view. Explanation of similarity between pre-existing low Organisms and those producible *de novo*. Fundamental cause of organic 'Reproduction.' Same Molecular Laws governing the production of Crystals and Organisms respectively. Transitions between not-living and 'living' matter. Highest modes of Molecular Composition in other Planets and Systems.

WHEN specks of living matter appear *de novo* in any fluid, they soon assume one or other of the shapes with which we are more or less familiar, just as specks of crystalline matter, of whatsoever nature they may be—whether previously known or altogether new—

fall into shapes belonging to one or other of the ordinary crystalline types of form.

But the prevailing shapes of crystals and of organisms respectively—those differences of form which are supposed to be characteristic and peculiar to each—are obviously referrible in part to the nature of the constituent molecules, and in part to the nature of the medium in which these molecules aggregate. The more closely the molecules of the crystalloid and the medium in which they unite approximate to colloidal complexity and the kind of media in which organisms are found, the more do the shapes of the crystalloid aggregates resemble those of the simplest organisms. This has been conclusively shown by Mr. Rainey's experiments; and they, together with other considerations now to be mentioned, almost compel assent to the correctness of the view already advanced by Mr. Spencer and several others, to the effect that the shape and structure of any organism is to be regarded as the result of the play of the molecular affinities of the organizable matter under the influence of the forces operative in its medium—that, in fact, organisms are produced owing to, and under the influence of, precisely the same laws as those which give birth to and regulate the form of crystals. Unless this be so, how are we to explain the various cases of restoration of lost parts in animals—how, in fact, are we to give an account of the phenomena of reproduction by fission or gemmation, in which mere isolated parts of an organism grow so

as to produce an organism similar to that from which they were derived? It is well known that minute fragments cut from a *Hydra* or from a *Medusa*—when placed in suitable situations—are capable of developing into perfect organisms similar to those from which they had been derived; just as a mere fragment of a crystal thrown into an almost super-saturated solution of the same salt will lead to the formation of a perfect crystal. We are told, moreover, by Dr. Hooker[1], that there is 'a species of *Begonia*, the stalks, leaves, and other parts of which are superficially studded with loosely attached cellular bodies,' and that 'any one of these bodies, if placed under favourable conditions, will produce a perfect plant similar to its parent.' The power of repair and reproduction of lost parts which is exhibited by many animals comparatively high in the scale of organization, is due to precisely the same causes. Multiplication by gemmation is, in fact, only an extreme form of the phenomenon which takes place when the crustacean reproduces a lost limb, when the lizard and the triton reproduce a tail that has been accidentally or purposely severed, or even when the fish reproduces a similar part[2]: both sets of

[1] 'Report of Brit. Association,' 1868.

[2] Facts of this kind have been only recently made known in respect to animals so high in the scale as fish. The following quotation ('Athenæum,' Aug. 19, 1871) refers to a communication made at the meeting of the British Association in Edinburgh:—'Prof. Traquair described two specimens of *Protopterus annectens*, in which the external configuration and internal structure rendered it evident that a consi-

phenomena are only explicable on the assumption that the form and structure possessed by each organism is that which is most consistent with the nature and properties[1] of the complex organic molecules, or par-

derable portion of the tail had been broken off; and that in the one case a less, and in the other a greater amount of restoration had taken place. . . . . . In the second specimen, which measured $9\frac{3}{4}$ inches in length, and had evidently been truncated or mutilated at a distance of about $7\frac{1}{2}$ inches from the tip of the snout, or $1\frac{3}{8}$ inch from the origin of the ventral fins, the restorative process had proceeded to a much greater length. Although the boundary between the old and new textures was sufficiently indicated on the outside of the fish, by the sudden diminution in the thickness of the specimen and in the size of the scales, the outline of the posterior extremity of the animal was very well restored, though the whole tail was still proportionately shorter than if no mutilation had taken place. The restored portion of the tail measured $2\frac{1}{5}$ inches in length, and on dissection showed not only, as in the former case, a reproduction of the notochord, but also of the neural and hæmal arches, spines, and fin-supports, these elements remaining, however, entirely cartilaginous, and being much more irregularly disposed than in the normal tail. They also cease to be traceable after $1\frac{1}{3}$ inch from the commencement of the new portion of the tail, though the notochord proceeds to its ultimate filiform termination. In addition the spinal cord, the lateral muscles, and the fin rays and their muscles were in this specimen reproduced as well as the scales on the external surface. Both externally and internally the line of demarcation between the old and new textures was distinctly seen.'

[1] Loc. cit., p. 181:—'For this property there is no fit term. If we accept the word *polarity* as a name for the force by which inorganic units are aggregated into a form peculiar to them, we may apply this name to the analogous force displayed by organic units. But, as above admitted, polarity as ascribed to atoms *is but a name for something of which we are ignorant*—a name for a hypothetical property which as much needs explanation as that which it is used to explain. Nevertheless, in default of another word, we must employ this; taking care, however, to restrict its meaning. If we simply substitute the term "polarity" for the circuitous expression, "the power which certain units

ticular 'physiological units,' of which it is composed. On this important subject Mr. Herbert Spencer says[1]:— 'We must infer that a plant or animal of any species is made up of special units, in all of which there dwells the intrinsic aptitude to aggregate into the form of that species: just as in the atoms of a salt there dwells the intrinsic aptitude to crystallize in a particular way. It seems difficult to conceive that this can be so; but we see that it *is* so. Groups of units taken from an organism (providing they are of a certain bulk and not much differentiated into special structures) *have* this power of rearranging themselves; and we are thus compelled to recognize the tendency to assume the specific form as inherent in all parts of the organism. Manifestly too, if we are thus to interpret the reproduction of the organism from one of its amorphous fragments, we must thus interpret the reproduction of any minor portion of an organism by the remainder. When in place of its lost claw a lobster puts forth from the same spot a cellular mass, which, while increasing in bulk, assumes the form and structure of the original claw, we can have no hesitation in ascribing this result to a

have of arranging themselves into a special form," we may, without assuming anything more than is proved, use the term "organic polarity" or "polarity of the organic units" to signify the proximate cause of the ability which organisms display of reproducing lost parts, or of their having assumed the shape and structure which is peculiar to them.'

[1] 'Principles of Biology,' 1864, vol. i. p. 181.

play of forces like that which moulds the materials contained in a piece of Begonia-leaf into the shape of a young Begonia. In the one case as in the other, the vitalized molecules composing the tissues show their proclivity towards a particular arrangement; and whether such proclivity is exhibited in reproducing the entire form, or in completing it when rendered imperfect, matters not.'

But the reader may ask, What is the meaning or explanation of this power of reproducing their like which is possessed by all living things?

In order to answer the question we must look rather to what occurs amongst the lowest organisms than to the phenomena presented by higher plants and animals. The fundamental nature of the process of reproduction is revealed most clearly by a consideration of the processes of 'fission' and 'gemmation.' What we know about these processes, clearly shows that all parts of a lower organism when separated from the parent have the power of developing into living things of a similar kind. This, as we have already pointed out, is precisely analogous to the process whereby a fragment *broken* from a pre-existing crystal and thrown into a suitable solution gradually grows into a perfect crystal, similar to that from which it had been derived. But organisms are dynamical aggregates amongst the molecules of which new motions and new arrangements are continually being assumed, in the course of which there frequently arises a 'spontaneous' division of

the parent mass—that is to say, fission or gemmation takes place. Nothing similar occurs in the crystal, because this is a statical aggregate in which no molecular rearrangements habitually occur. The tendency which the molecules display to grow into a given form is, however, not much more manifest in the crystal than it is in the organism. The fundamental difference between the two lies in the fact that the one is a *statical* and the other a *dynamical* aggregate. As a result of this difference, we find that the growth of the one is always continuous, whilst that of the other is frequently discontinuous—a 'spontaneous' separation of a portion of its substance may, and frequently does, take place in the case of the growing organism whereby self-reproduction is brought about[1].

[1] Referring to the products of the multiplication of a single germ, Mr. Herbert Spencer points out that 'total insubordination among the centres of development is shown where the units or cells, as fast as they are severally formed, part company and lead independent lives. This, in the vegetable kingdom, habitually occurs among the *Protophyta;* and in the animal kingdom, among the *Protozoa.* Partial insubordination is seen in those somewhat advanced organisms that consist of units which, though they have not separated, have so little mutual dependence that the aggregate they form is irregular. Among plants, the Thallogens very generally exemplify this mode of development. Lichens, spreading with flat or corrugated edges in this or that direction, as the conditions determine, have no manifest co-ordination of parts. In the *Algæ,* the Nostocs similarly show us an unsymmetrical structure. Of *Fungi,* the sessile and creeping kinds display no further dependence of one part on another than is implied by their cohesion. . . . To distinguish that kind of development in which the whole product of a germ coheres in one mass from that kind of development in which it does not, Professor Huxley has introduced the words "*continuous*" and "*discontinuous;*"

Thus 'reproduction' amongst such simple organisms is, after all, nothing but *discontinuous* growth. This simple interpretation of the distinctive peculiarity of living things was long ago pointed out by Prof. Huxley; and the fact that growth is frequently discontinuous in living matter flows directly, as we have hinted, from the circumstance of the extreme molecular mobility of its constituent units. All the higher modes of reproduction which are to be witnessed in living things are only specializations of the process of 'gemmation.'

By reason of the molecular mobility of 'living' matter, and the continuous rearrangements brought about therein under the influence of ordinary physical conditions, it gradually becomes more and more complex in internal structure, and also undergoes variations in its external form [1]. But to whatsoever grades of development organisms may have attained, the reproductive faculty (due to the power of discontinuous growth) still remains as one of the chief characteristics of living things. And it always happens, that suitable portions thrown off from the parent organism have the power of developing into organisms of a similar kind—however complex and

and these seem the best fitted for the purpose. Multicentral development, then, is divisible into continuous and discontinuous.' ('Principles of Biology,' vol. i. p. 135.)

[1] Abundant evidence on this subject will be found in *Appendix* D. Many of the changes there recorded far surpass the metamorphoses brought about in such crystals as those of mercuric iodide under the influence of new conditions—though they are otherwise comparable with these metamorphoses.

however long the developmental processes may be which have to be passed through before the parent form can be assumed. The human ovum develops as surely into a human being, as that of a fish does into a similar fish. The process is, in fact, everywhere the same—whether we have to do with mammal, bird, reptile, or fish, or whether the recently separated portion of an *Amœba*, of a *Hydra*, or of some plant undergoes development. In each case there is a reproduction of like from like, quite irrespective of the grade of development which has been attained by the parent organism. These phenomena have been generalized under a 'Law of Heredity,' whose meaning (after the most careful consideration of the facts) has been thus admirably rendered by Mr. Herbert Spencer. He says[1]:—'Bringing the question to its ultimate and simplest form, we may say that, as on the one hand physiological units will, because of their special polarities, build themselves into an organism of a special structure; so, on the other hand, if the structure of this organism is modified by modified function, it *will impress some corresponding modification on the structure and polarities of its units.* The units and the aggregate must act and re-act on each other. The forces exercised by each unit on the aggregate and by the aggregate on each unit must ever tend towards a balance. If nothing prevents, the units will mould the aggregate into a form in equilibrium with their pre-existing polarities. If, con-

[1] 'Principles of Biology,' vol. i. p. 256.

trariwise, the aggregate is made by incident actions to take a new form, its forces must tend to remould the units into harmony with this new form. And to say that the physiological units are in any degree so remoulded as to bring their polar forces towards equilibrium with the forces of the modified aggregate, is to say that when separated in the shape of reproductive centres, the units will tend to build themselves up into an aggregate modified in the same direction.'

Amongst simple organisms almost any part of the substance which separates, or is separated, from one of them is capable of developing into a similar simple organism. But as organisms grow more and more complex in their structure, so we find that a difference arises in the reproductive powers of different tissues—till at last the capacity to reproduce the entire organism (either without fertilization or only after this has occurred) becomes restricted to the morphological units which are produced in special organs [1]. How much this restriction of the reproductive function is due to a general specialization is obvious from the fact that it is most marked where complexity of organization attains its maximum. Complexity of structure necessarily carries with it complexity of function, and in proportion as distinct functions

[1] The necessity for the fertilization of some of these reproductive elements, and the evolution of sexual differences amongst the animals and plants amongst which this necessity obtains, is merely a superadded complexity—a difference of degree and not of kind. The fundamental phenomena of reproduction are essentially similar in sexual and sexless organisms. (See Spencer's 'Principles of Biology,' vol. i. pp. 218–223.)

are performed by special parts of the organism, so are the several parts more and more bound together into one organic whole. This difference is well seen in plants and highly-organized animals. The plant, it is true, develops seeds and pollen in special parts or organs; but just as the plant, taken as a whole, is to a great extent a repetition of similar parts whose organization is by no means complex, so do these separate parts, when severed from the parent organism, retain that reproductive power which enables them, under suitable conditions, to grow into plants of a similar kind. But in animals which are even low in the scale of complexity all this is changed. They are not mere repetitions of separate parts—each having a potential individuality of its own: they are rather aggregations of different parts bound together and constituting one organic whole by means of vascular and nervous systems which serve as bonds of unity. The higher the grade of development of the organism—the more its tissues have become differentiated—the less are they severally endowed with a reproductive power, even of a partial kind. In such organisms, we find that each part has a distinct function to perform, and therefore the reproductive function is restricted to the elements produced in definite organs. Although restricted in their place of origin, however, there is reason to believe that sperm-cells and germ-cells are comparatively unspecialized products—they are, indeed, in almost all cases new-formed elements, which have

but recently come into being[1]. After a careful summary of what is known on the subject, Mr. Spencer says:—'The assumption to which we seem driven by the *ensemble* of the evidence is, that sperm-cells and germ-cells are essentially nothing more than vehicles in which are contained small groups of the physiological units in a fit state for obeying their proclivity towards the structural arrangement of the species they belong to. . . . . Thus, the phenomena of Heredity are seen to assimilate with other phenomena; and the assumption which these phenomena thrust on us appears to be equally thrust on us by the phenomena of Heredity. We must conclude that the likeness of any organism to either parent is conveyed by the special tendencies of the physiological units derived from that parent. In the fertilized germ, we have two groups of physiological units, slightly different in their structures. These slightly different units severally multiply at the expense of the nutriment supplied to the unfolding germ, each kind moulding this nutriment into its own type. Throughout the process of evolution the two kinds of units mainly agreeing in their polarities and in the form which they tend to build themselves into, but having minor differences, work in unison to produce an organism of the species

[1] See vol. i. pp. 199–207. We have already pointed out the great evolutional capacities that seem to be possessed by the phosphoric fats, which enter so largely into the composition of ova (vol. i. p. 212). These fats are closely allied to myeline and other remarkable fatty extracts (see p. 118).

from which they were derived, but work in antagonism to produce copies of their respective parent organisms; and hence, ultimately, results an organism in which traits of the one are mixed with traits of the other.' (p. 254.)

Unless we have recourse to considerations of this kind, or to some such hypothesis as that which Mr. Darwin has put forth under the name of 'Pangenesis[1],' it seems absolutely impossible for us to give any explanation of the familiar fact, that in the ordinary processes of reproduction, all organisms, whether high or low in the scale of complexity—animal, vegetal, or protistic—produce offspring which more or less directly develop into organisms similar to themselves. Very grave difficulties appear to stand in the way of Mr. Darwin's hypothesis, which looks like a relic of the old, rather than a fitting appanage of the new Evolution philosophy[2]. On the other hand, Mr. Spencer's totally different hypothesis concerning 'physiological

[1] 'Plants and Animals under Domestication,' vol. ii. p. 357. Mr. Darwin says:—'Gemmules are supposed to be thrown off by every cell or unit, not only during the adult state, but during all the stages of development. Lastly, I assume that the gemmules in the dormant state have a mutual affinity for each other, leading to their aggregation either into buds or into the sexual elements. . . . Hence ovules and pollen-grains,—the fertilized seed or egg as well as buds,—include and consist of a multitude of *germs thrown off from each separate atom of the organism*.' So that Pangenesis 'implies that the whole organisation, in the sense of every separate atom or unit, reproduces itself' (pp 374 and 358).

[2] See an article entitled 'Darwin's Hypotheses,' by Mr. G. H. Lewes, in 'Fortnightly Review' for 1868, and Mr. St. George Mivart's 'Genesis of Species' (1871), chap. x.

units' seems to be supported by many facts otherwise inexplicable, and to be altogether in harmony with general biological principles, and with the modern Evolution hypothesis—as opposed to that of Bonnet respecting the continual unfolding of pre-existing germs.

We may say, therefore, that 'inheritance,' acting in the manner above indicated, is the potential conservative agency tending to assimilate the products of reproduction to the likeness of the organisms from which they have been produced. But where simple organisms are exposed to changes in their environment, they are, by virtue of these changes, subjected to influences which may be capable of inducing functional and structural modifications. Great differences, however, exist with respect to the degree of variation that may be induced in different organisms within similar periods, under the influence of any given changes in their environments. Changes, which may be almost inoperative in producing a modification of some organisms, may produce profound alterations in others. And, similarly, whilst a very prolonged continuance of altered conditions is needful to effect some organisms, the influence of changed conditions on others is rapid and more or less immediate.

The greater the differentiation and complexity of any organism, the less is it likely to be influenced by slight or temporary modifications in the 'conditions' or influences to which it is subjected. The complexity has been gradually attained, and each part or organ has functions

and structures which are definitely related to the functions and structures of other parts. The whole is composed of parts working in accord with one another, and in such a manner as to establish a harmony between the actions going on within and those without the organism. The result of this interaction during past time has been the gradual elaboration of an organism of a certain structure; so that this structure and form are only to be regarded as the physical expression of an approximate equilibrium between numerous related factors—between the inherent tendencies of 'physiological units' under the influence of all past 'conditions,' and the present operation of external forces upon the now-acquired structure. When, therefore, unaccustomed conditions act upon such organisms, they are unable easily or within short periods to produce direct modifications of the organs principally affected, because a change in one important organ would necessitate other changes throughout the whole organism, in order to establish a new balance of functions. This, in fact, is the only conclusion which seems consistent with doctrines of Evolution. Mr. Spencer says:—'If we assume, as we must according to this hypothesis, that the structure of any organism is the product of the almost infinite series of actions and reactions to which all ancestral organisms have been exposed, we shall see that any unusual actions and reactions brought to bear on an individual can have but an infinitesimal effect in permanently changing the structure of the

organism as a whole. The new sets of forces compounded with all the antecedent sets of forces can but inappreciably alter that moving equilibrium of functions which all these antecedent sets of forces have established. Though there may result a considerable perturbation of certain functions—a considerable divergence from their ordinary rhythms—yet the general centre of equilibrium cannot be sensibly changed. On the removal of the perturbing cause, the previous balance will be quickly restored, the effect of the new forces being almost obliterated by the enormous aggregate of forces which the previous balance expresses[1].' Thus, variations which may be induced for a time in higher organisms, continually tend, when the modifying influences have disappeared, to be dwarfed and perhaps ultimately abolished, owing to the sum total of internal forces acting in such a manner as gradually to reproduce the former condition of equilibrium[2].

But how different are the facts when we turn our attention to lower organisms! Almost all naturalists admit the greater amount and range of variability amongst the lower forms of life, although we think

[1] For a fuller explanation of the reasons why this should be than we are able now to enter upon, we must refer the reader to Mr. Spencer's 'Principles of Biology,' vol. i. pp. 192–198.

[2] It is this tendency which was formerly spoken of in medical works as the *vis medicatrix naturæ*. Were it not for such a tendency, the success of the physician in combating with internal diseases, or of the surgeon in superintending wounds and injuries, would be much less manifest than it is at present.

that a perusal of *Appendix* D (in which we have given an account of some of the most remarkable and well-attested variations of these lower forms) may perhaps bring more prominently before many of our readers the full extent of this plasticity than if they were to read mere isolated statements. We have seen *how the structure and nature of certain crystals may be* wholly changed by some modification in the conditions to which they are subjected. We have learned, in fact, how potent, within certain limits, is the influence of the forces of the environment, in comparison with that comprised under the head of 'inherent tendencies,' in determining crystalline form; so that if a similar characteristic should be displayed by new-born living matter, or by simple organisms, this would be merely a verification of what might have been predicted *à priori.*

We have now frequently stated that the essential characteristic of living matter is its extreme plasticity, and its power of carrying on a reciprocal series of changes and rearrangements amongst its constituent molecules in response to changes in its medium or environment. Its history is one of continual flux and change. And seeing that those causes which operate in checking the modifying influence of external conditions in complex and more completely individualized organisms, do not come into play in the most elementary organisms or in others which are made up to a large extent by a repetition of similar parts, we might expect that the latter would be prone to undergo

change, other things equal, in direct proportion to the simplicity of their organization. We are driven to the conclusion, in fact, that the simpler the organisms with which we have to deal, the weaker will become the influence of Heredity; and that in independent or new-formed living matter this conservative tendency would be no more potent than it is in crystalline matter.

This legitimate conclusion once arrived at, the consideration of a most important problem is forced upon us. What explanation, it may be asked, is to be given of the existence of multitudes of the lower forms of life at the present stage of the earth's history? It seems only possible to account for their presence by one or other of two explanations. Each of these we will briefly consider.

1. *Theory of Homogenesis.*

Those who disbelieve in the occurrence of Archebiosis and of Heterogenesis—the disciples and advocates, in fact, of the biological doctrines at present most widely accepted—would have us believe that no independent evolution of life has taken place upon our globe since a period shrouded in the far-remote depths of geologic time. Since many of these are willing to admit that progressive specific transitions have continually been taking place through these ages, and that all the various forms of life which have ever clothed the earth are to be considered as representatives of such developmental

variations[1], it is only to be expected that they should attempt to offer some explanations as to the cause of the persistence of so many of the very lower forms—why, in fact, these particular representatives have not undergone any notable evolutional changes during this long succession of ages. We are told by Dr. Carpenter[2] that 'there is strong reason to regard a large proportion of existing Foraminifera as the ***direct lineal descendants*** of those of very ancient geological epochs,' on account of the great resemblance existing between the fossil remains of the latter and the organisms met with at the present day. He thinks their progenitors may be traced back even as far as the upper Triassic rocks. If we turn now to the reasons offered for this long-continued essential similarity, we find Dr. Carpenter writing as follows:—'It can scarcely be questioned that such a continuity of the leading types of Foraminifera, maintained through so long a series of geologic periods, and the ***recurrence*** of similar varietal departures from these types, are results of the facility with which creatures of such low and indefinite organization adapt themselves to a great diversity of external conditions; so that, on the one hand, they pass unharmed

[1] Instead of having recourse to a special creative fiat or miraculous intervention, in order to account for the presence of each separate kind of animal or plant. For a comparison of the evidence bearing respectively upon these two hypotheses, we may refer the reader to Mr. Spencer's 'Principles of Biology,' vol. i. pp. 333–345.

[2] In the very interesting *Preface* to his 'Introduction to the Study of Foraminifera.' (Ray Society, 1862, p. viii.)

through changes in those conditions which are fatal to beings of higher structure and more specialized constitution; whilst, on the other, they undergo such modifications under the influence of those changes as may produce a very wide departure from the original type.' These views now seem somewhat inconsistent and contradictory. Extreme variability is predicated on the one hand, and yet extreme stability is affirmed to have been displayed through long geologic ages. Doubtless, the 'conditions' obtaining at the bottom of deep oceans in past times may not have been very different from what they are at present[1]; but such uniformity of conditions could not entail the long-continued preservation of the same simple structural types, unless we suppose that all internal causes of change in the organisms themselves had ceased to exist. And yet the continuous existence of internal causes of change is, in reality, the essential attribute of living matter, which could no more have been absent from Foraminifera during all these ages of apparent non-development, than it is absent at the present day in the ever-varying Fungi, Algæ, and Lichens, which astonish us by their rapid and protean changes of form. It is certain, moreover, that those who believe exclusively in the

[1] If certain lower organisms, therefore, developed into Foraminifera in remote geologic ages, there is no reason why they should not develop in the present day into essentially similar forms; and variation may now tend to manifest itself in the same fashion as it did formerly, owing to the fact that the causes (both intrinsic and extrinsic) leading to this variation are essentially similar.

'continuity of life' by processes of Homogenesis (and are yet sufficiently scientific to reject, as untenable and absurd, the hypothesis of special creative fiats) would be compelled to believe that the simple Monad which now lashes about in an organic infusion, or the almost structureless Amœba which now creeps amongst decaying vegetable tissue, must be derived from an incalculably longer line of ancestry than Man himself. In accordance with any evolution hypothesis, Man must be considered as a comparatively recent organic form; and those whose views are at present most widely accepted would have to believe that ages and ages before the advent of Man or his immediate predecessors upon the earth, the ancestry of the Monad, of the Mould, of the Amœba or of any of the other Infusorial animalcules now to be seen in their respective habitats, had been tenants of our globe. The mere suggestion seems to carry absurdity in its face. If this were really so, then we could only expect that such forms would be the very types of conservatism and stability, whereas, as a matter of fact, all such organisms are rather the best types of change and mutability. On this subject there is a general unanimity of opinion; and the very existence of the changes which are now known to take place, seems absolutely antagonistic to the doctrine of the direct lineal descent of the present lowest and most variable forms of life, from similar extremely ancient forms possessed of a like mutability.

The more we reflect upon it, in fact, the more im-

possible does it seem that this theory can be true. All developmentalists would start in bygone ages with living matter existing in its simplest form or forms; and this must be supposed to be far more mobile and changeable than crystalline matter. How this latter may vary under the influence of changing conditions we have already shown: we may fairly expect, therefore, that the newly-evolved, primordial, living matter would be even more prone than this has been shown to be, to assume new developmental forms under the influence of changing external conditions. We see our way, therefore, quite plainly to an advance in the developmental scale, and, owing to the tendency of organisms to reproduce their like (under the influence of Heredity), we may understand how a continual widening of the successive platforms may take place, upon some parts of which further developmental differentiations may be initiated. In their turn, the various different and often more complex forms thus produced, are multiplied by further reproductive processes, and so on through innumerable series and ramifications,—organisms being gradually evolved whose complexity makes it more and more difficult to bring about permanent alterations, even when aided by the powerfully modifying agencies included under the head of 'Natural Selection.' Longer and longer periods become necessary to induce even slight specific alterations. Change, therefore, is rapid amongst the lower terms of the series, and more and more slow as we ascend in the scale of complexity and

individuation. All this seems to follow naturally from the mutability of new-born living matter on the one hand, and that conservative tendency (due to Heredity) which increases with complexity of organization. Mutability being the essential quality of living matter, as such, it would seem almost impossible for us to believe that whilst some of the lowest organisms, such as Moulds and Amœbæ, developed into forms continually higher and higher, others should, in spite of their intrinsic mutability (which present observation shows to be retained), persistently preserve the same almost primordial simplicity as their ancestors possessed in an incalculably remote past. That some Moulds, Amœbæ, and other lowest organisms should have lived in unbroken continuity through pre-Silurian epochs, amidst all the changes of the Carboniferous, Triassic, Oolitic, Cretaceous, and more recent geologic ages, with that mutability as an essential characteristic which they are now seen to display, and yet that they should have undergone little or no alteration, seems to me almost too incredible to be seriously entertained.

But what view can be substituted which will offer a more consistent explanation of the facts?

2. *Theory of Archebiosis, Heterogenesis, and Homogenesis.*

Guided by the results of experimental evidence, we are entitled to believe that 'living' matter can and does continually come into existence, owing to the synthesis and gradual elaboration of not-living mate-

rials. What takes place now has probably taken place in all intermediate time, between the present epoch and that of the first appearance of Life upon our globe. The variability of this new-born living matter and of the lowest forms evolved therefrom is most marked, and probably has always been so in past ages. In all periods, therefore, whilst there have been forms of life, both animal and vegetal, of varying degrees of complexity and of varying stability, reproducing their kind by the different modes of homogenetic reproduction, there have always been a teeming multitude of more variable and plastic forms, more or less immediately developed from the new-born living matter which is continually springing into existence. These lower forms, produced in the same epoch, differ in accordance with the conditions by which they are surrounded, and the variations in the exact nature of the molecules from which they arise; some of these first forms *may* have differed notably from epoch to epoch, and we are by no means to conclude that all the primordial forms of to-day are similar to those which made their appearance during the Silurian or Carboniferous ages, when the sum total of telluric conditions, capable of influencing such lower forms, may have been so different[1]. All that

[1] Hence, there may have been many different starting-points, along which evolution may have progressed to a certain though variable extent. This view lessens the difficulties of the naturalist, who is no longer bound to look upon all animals and plants as members of a single imperfect series. The routes which have given rise to all the

we are entitled to say is, that *Bacteria*, *Torulæ*, and *Amœbæ* are known to be some of the present primordial forms of life. Being primordial, all theoretical considerations would lead us to believe that these organic forms would be as variable as careful observation has shown them to be; and that a similar variability, gradually growing less and less, would characterize the different forms into which they might chance to develop. This, also, is thoroughly in accordance with the experience of those who have made such forms of life the subject of long and careful study[1].

If, then, new-born living matter may develop at once into different kinds of *Torulæ* and thence into Fungi—or else into essentially similar organic forms only after it has passed through the intermediate grades of *Bacteria* and *Leptothrix* filaments,—does it not almost certainly indicate that there must be some harmony between the structure and modes of growth of such lower kinds of Fungi, and the sum total of conditions by which they are influenced? But, during their growth, and as a natural consequence of their intrinsic molecular movements, these simple organisms may *throw* off buds and segments which (like portions *broken* off from a crystal)

known forms of life may have been many, and the different evolutional series may have been arrested at different stages, or may be still in progress.

[1] To what extent this variability is met with, and how complex are the organisms which may spring into existence without previous parents of the same kind, will be shown in subsequent chapters, and in *Appendix* D.

always tend, if left under the same conditions, to develop into structures similar to those from which they had been derived.

Thus we shall find that it is but a direct consequence of their very nature and mode of growth that many Fungi develop various kinds of 'fructification.' And in this case we find that portions (usually called 'conidia' or 'spores') into which the living matter divides or buds, have the power—when separated from the parent—of developing, under similar conditions, into organisms which resemble those from which they had been derived. Why do they possess such a power? Because, being fragments of living matter formed in, or as parts of, an organism of a given character, they have derived from it (or 'inherited') certain developmental capacities by which, when separate, they are enabled to grow into organisms like the parent—just as a bud from the surface of *Hydra viridis* will develop into another polype of the same kind. The parent Fungus assumed the organic form which it possessed, because such form had been the joint and necessary product of the 'conditions,' acting upon the particular molecular mobilities and modes of growth of plastic new-born living matter. Mr. Spencer says[1]:— 'As certainly as molecules of alum have a form of equilibrium, the octahedron, into which they fall when the temperature of their solvent allows them to aggregate, so certainly must organic molecules of each

[1] 'Appendix to Principles of Biology,' p. 487.

kind, no matter how complex, have a form of equilibrium in which, when they aggregate, their complex forces are balanced,—a form far less rigid and definite, for the reason that they have far less definite polarities, are far more unstable, and have their tendencies more easily modified by environing conditions.' And portions of living matter which separate from a developed Fungus go along a similar developmental groove, when they lead an independent existence in a similar medium for the same reason that the new-born living matter assumed this particular form. They grow at once into the form of the parent, because this parental form has been the result of the harmonious action of the same series of actions and reactions as are now about to be passed through. Similar causes (intrinsic and extrinsic) should lead to similar results.

If it is true, as we affirm, that *Torulæ*, and even *Bacteria*, are enabled occasionally to develop directly into simple forms of Fungi, and that such *Bacteria* and *Torulæ* are merely the primary forms most frequently assumed by certain kinds of new-born living matter, then obviously the form and structure of the Fungus would stand in the same relation to the matter of which it is composed that the form and molecular structure of the crystal does to its matter. There would be, in fact, just as much reason why the new-born organism should develop into the form of one already in existence, as there would be that the crystal of sulphate of soda which forms to-day in a solution

of that substance, should resemble that which formed under similar conditions twelve months or two years previously. He who believes in the uniformity of natural phenomena could anticipate no other result. Living matter which is now produced *de novo*, speedily shapes itself into some well-known form; and so also new crystalline matter which may have been produced synthetically by the chemist in his laboratory, falls habitually into one or other of the known crystalline systems.

It seems, again, no more wonderful that the organism which develops *de novo* to-day should resemble another which develops from the 'spore' of a pre-existing organism, than that a crystal which forms to-day in a saline solution should resemble another which is capable of arising by the growth of a portion detached from a similar pre-existing crystal. In all these cases, there is a similarity of product, because the crystalline or organic form produced is to be regarded as the physical expression of the harmonious actions which have led to their production—because the forms are the results of a physical necessity, and not of a mere blind chance[1].

[1] And yet an objection has been gravely raised by an eminent biologist to receiving the conclusion which I was inclined to draw from some of my experiments, on account of the extreme difficulty he experienced in believing that the same simple Fungi could be produced from new-born living matter as were known to be produced in other ways—that is, by some of the methods of reproduction to which we have just been alluding. If it be supposed that such organisms have been reproducing after this

And whilst new-born crystalline matter is capable of developing at once into complex but perfect geo-

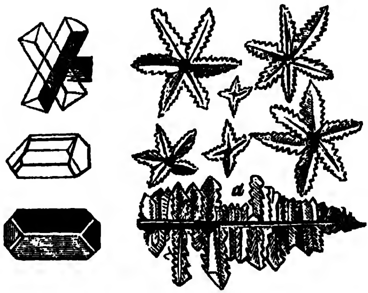


FIG. 44.
Different Forms assumed by Crystals of Ammoniaco-magnesian Phosphate. (Dr. Beale.)

metrical forms, or of assuming many varied and beautiful shapes which are common in the vegetable world,

fashion since the first advent of Life upon our globe—that those at present in existence are the direct lineal descendants of certain archaic forms, as seems to be tacitly intimated—there might be more difficulty in explaining the coincidence. But, perhaps it would have been better if this critic had realized the fact that, in the event of living matter having really arisen independently in the experimental flasks, it would entail the necessity of diminishing almost to zero the accredited pedigree of these low kinds of Fungi. Had this been thought out a little more fully, such an eminent biologist would perhaps have hesitated before he made the following sensational declaration to a large audience :—'If it is true that Torula forms, Bacteria forms, and such things which are common products of so-called spontaneous generation —if these can be shown to be terms in the development of a known form—the probability of the same identical form turning up spontaneously becomes by *mathematical considerations* infinitely minute; and for my part, I could *as soon believe that the calf I see grazing in a meadow had been spontaneously generated from the grass and flowers there.*' ('Quart. Jrnl. of Microsc. Sc.,' Oct. 1870, p. 361.)

some would have us believe that the more complex and mobile specks of new-born living matter, with all their power of undergoing continuous internal change, ought nevertheless to assume no specific shapes—*Bacteria* ought not to develop into *Vibriones* and *Leptothrix* filaments, and none of these ought to grow into mycelial filaments; or a *Torula* cell, which is more slowly evolved in the same fluids, ought not to bud out and

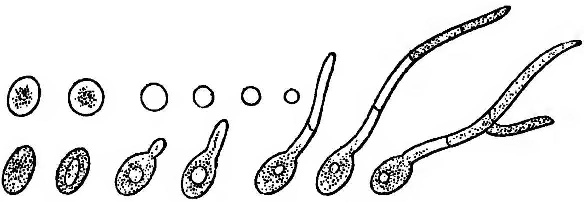


Fig. 45.

Evolution of a primordial speck of living matter, through *Torula*-forms, into Fungus filaments. (Pouchet.)

grow into similar filaments, so as to give rise to the simpler forms of Mould. These seem to be the views of many who have been pleased to criticise my experiments. Living matter is admitted to be more complex than crystalline matter, and to be endowed with the power of undergoing continuous internal molecular changes; and yet, whilst crystalline matter may and does develop into the most beautiful and complex crystalline forms, new-born living matter, it is said, ought not to evolve at once into the simplest kinds of organisms. All this may seem true to some, but I must

confess my own utter inability to understand what reasons can be adduced in support of so contradictory a doctrine. Reason alone might lead to its rejection, even if careful and repeated experiments had not shown it to be erroneous.

No reproductive elements are cast off by the crystal, because this is a statical aggregate which undergoes no *continuous* series of molecular actions. Its constituent units always tend to fall into a condition of polar equilibrium; and when occasional changes occur, they are always due to extrinsic agencies. Reproductive elements are, however, frequently and of necessity thrown off from the organism, because its polarities are often too complex to admit of an equilibrium being established: a current and continuous molecular re-arrangement goes on, as a result of which, when an approximate equilibrium is otherwise impossible, certain portions of its constituent matter tend to aggregate round new centres, which become independent and ultimately by a continuance of the same action separate from the parent organism—as 'conidia' or 'spores.'

These views, which flow as a necessary consequence from the doctrines of evolution, have now, by the results of the experiments detailed in previous chapters, received the only warrant which was needed. Having learned that new living matter can originate *de novo* after the same fashion as new crystalline matter, the only doctrine which at present seems open to us is that which has just been explained. The forms and

structures of simple organisms, like the forms and molecular structures of crystals, are referrible to the action of universal and immutable physical laws.

All the evidence we have gathered together in this and preceding chapters alike tends to show, that the differences which exist between various kinds of matter depend in the main upon differences in molecular structure or mode of aggregation. This conclusion is forced upon us by the phenomena of allotropism and isomerism, by the consideration that thousands of wholly different substances are compounded merely of carbon, hydrogen, and oxygen in similar or different proportions, and by multitudes of other facts of a like nature. Some of these aggregates are stable, whilst others are highly unstable. Slight external influences suffice to alter the crystalline form of certain bodies, some of which, such as mercuric iodide, undergo the most remarkable changes. Such alterations are all passages from one mode of statical aggregation to another mode of statical aggregation. And yet crystalline matter is often capable of undergoing a very different kind of rearrangement by which it is converted into a colloid. The colloid is distinguished by its extreme mutability—its existence is a continual metastasis. It is, in fact, a dynamical state of matter. Further aggregations and rearrangements may take place amongst its molecules and give rise to other forms of matter possessing the mutability which distinguishes

colloids even in a more eminent degree, and to such an extent as to enable them to carry on a *continuous* series of molecular changes in response to the incidence of mere ordinary physical forces. This, however, is but a further degree of complexity in a direction already indicated. All intermediate degrees of molecular mobility may be traced (amongst various crystalline and colloidal states of matter) between the distantly successive changes from one to another mode of polar equilibrium—which is alone possible with the majority of crystals—and the continuous changes of living matter[1]. The lapse from one mode of statical equi-

[1] A very good instance of such an intermediate product is to be found in the remarkable substance called 'myeline,' which, when immersed in water, protrudes delicate tubes that bend in all directions (Montgomery, 'On the Artificial Formation of so-called Cells'). But, as Robin has shown, similar or even more remarkable movements and changes of form (not due to contractility in its ordinary sense) are to be seen in other fatty extracts derived from dead animal substances. (See 'Mem. de l'Acad. de Médecine,' 1859, p. 248.) This is especially the case with fatty extracts obtained from the blood, when they are mixed with water or albuminous fluids:—' Des amas de ces *extraits*, on voit aussi sortir et s'allonger sous les yeux de l'observateur des filaments d'aspect tubuleux, prenant des dispositions rectilignes, coudées, onduleuses ou spiroïdes, analogues à celles de divers éléments anatomiques; parfois l'extrémité de certains de ces tubes se resserre, devient monoliforme, et les resserrements vont jusqu'à produire une scission avec séparation complète d'un globe creux, comme dans le cas de production des spores à l'extrémité des cellules tubuleuses de divers champignons, oïdiés, etc. . . . . Lorsque ce sont des gouttes sphériques ou à contour sinueux qui se sont formées, on peut les voir sous le microscope non pas s'infléchir dans un sens et dans l'autre, comme les filaments tubuleux précédents, mais changer de forme incessamment, par suite de resserrements et de dilatations alternatifs de certaines de leurs parties. Ces resserrements vont même

librium to another mode of statical equilibrium, if it take place with sufficient rapidity and be associated with a concurrent process of growth, will give rise to that continuous series of molecular changes which characterize what we know as 'living' matter. And yet the molecular aggregate which displays this continuous responsive mobility and power of self-division—because it has been called a 'living' thing, and because theoretical notions have been formed concerning 'life'—has been supposed to be separated from other closely-related kinds of matter by an impassable gulf.

Irresistibly the thought arises which may prompt us to ask, What higher modes of aggregation exist on the surface of other planets belonging to our own or different solar systems? Is it necessary to suppose that the highest modes of molecular aggregation which have appeared upon our earth, are at all similar to those which have arisen under the continued influence of more or less dissimilar conditions? May we not rather suppose, that other highest modes of aggregation may exist totally different from our own, each of which, to whatsoever planet or system it belongs, may possess qualities so subtle as to make it comparable only with that which we call 'living' matter?

jusqu'à produire une division complète de certains globules en deux, de la même manière qu'on voit s'opérer la scission par étranglement graduel de certaines cellules végétales et animales.' (Robin, 'Traité du Microscope,' 1871, p. 562.)

CHAPTER XV.

DEVELOPMENT OF THE PRIMORDIAL FORMS OF LIFE: THEIR RELATIONSHIP TO ONE ANOTHER.

Evolution. Simple and Compound. States of Matter favourable to Compound Evolution. Living Matter. Its Qualifications and its Changes. Von Baer's Law. Tendency to Differentiation. This observed to be a Tendency to 'Organisation.' It is inherent in 'living' Matter. Present Changes likely to be similar to Antecedent Changes. Rate of Variation. Different according to Mode of Nutrition. Vegetal and Animal Forms. Hints concerning Nutrition of Amœbæ.

Nature of Bacteria. Their relationship to Fungi. Relations between Bacteria and Torulæ. Causes which promote their 'discontinuous' Growth. Development of Bacteria into Fungi. Many different forms of Torulæ. These forms may 'breed true' but are still Interchangeable. M. Trécul's observations on Development of Torulæ. Variability at all Stages. M. Pouchet's Observations. Interchangeability of Lower Fungi. New-born Matter may assume the forms of Amœbæ or Monads. Mutual relationship between these and Fungi. Green Organisms. Transitions between Fungi and Algæ. Similar transitions between Fungi and Lichens. Relations of Desmids to Algæ. Fundamental Kinship of all these forms actually proved by Experiments. More Varied Forms which appear in Ponds. Their Transformations. Dr. Braxton Hicks on relationship between Algæ, Lichens, and Mosses. Cohn's Researches concerning Transformations of Protococcus. They tend to abolish Classificatory Distinctions. Views previously announced by Professor Grant.

'THE change from a diffused imperceptible state to a concentrated perceptible state is an integration of matter and concomitant dissipation of

motion; and the change from a concentrated perceptible state to a diffused imperceptible state is an absorption of motion and concomitant disintegration of matter. These are truisms. Constituent parts cannot aggregate without losing some of their relative motion, and they cannot separate without more relative motion being given to them[1].' To the former process Mr. Spencer applies the term Evolution[2], and to the latter Dissolution. Both processes are constantly being carried on, either separately or conjointly, in all existences whatsoever.

But there are two modes of Evolution which we shall do well to distinguish in their most divergent aspects, although they are connected with one another by almost insensible gradations.

When the forces at work are strong and tend to produce rapid aggregation, as in the case of the formation of a Crystal, we have to do simply with an 'integration of matter and concomitant dissipation of motion;' but when integration takes place more slowly, 'either because the quantity of motion contained in the aggregate is relatively great; or because though the quantity of motion which each part possesses is not relatively great, the large size of the

[1] 'First Principles,' 2nd ed. p. 284.

[2] This use of the word 'Evolution,' although arbitrary and open to many objections, was rendered inevitable by previous use and custom. As Mr. Spencer says:—'The antithetical word Involution would much more truly express the nature of the process' (loc. cit., p. 285).

aggregate prevents easy dissipation of the motion; or because though motion is rapidly lost more motion is rapidly received, then other forces will cause in the aggregate appreciable modifications. Along with the change constituting integration there will take place supplementary changes. The Evolution instead of being simple will be compound[1].'

What usually occurs in the latter case is indicated by the following definition[2]:—'Evolution is an integration of matter and concomitant dissipation of motion; during which the matter passes from an indefinite incoherent homogeneity, to a definite coherent heterogeneity; and during which the retained motion undergoes a parallel transformation.'

The presence of much retained internal motion in an aggregate undergoing condensation, is the peculiarity that, above all others, favours the occurrence of the internal changes and re-arrangements which characterize compound evolution. Yet whilst matter still exists in those states in which it contains the largest amount of this retained motion—whilst it is still in the gaseous or liquid stages—the secondary redistributions which undoubtedly take place vanish almost as soon as they occur—'the molecular mobility being such as to negative the fixed arrangement of parts we call structure[3].' But on approaching solidity 'we arrive at a condition called plastic, in which

[1] 'First Principles,' 2nd ed. p. 287.
[2] Loc. cit., p. 396.
[3] Loc. cit., p. 297.

redistributions can still be made, though much less easily; and in which, being changeable less easily, they have a certain persistence—a persistence which can, however, become decided only where further solidification stops further redistribution.'

Now molecular motion is locked up in living matter in various ways. In addition to the fact of its semi-fluid consistence, it contains chemical combinations which even surpass those of the colloid molecule in intrinsic mobility. Three out of its four principal ultimate constituents are mobile gases; whilst it is a peculiarity of one of them, nitrogen, that instead of giving out heat when it combines with other elements, it absorbs heat, so that 'besides carrying with it into the liquid or solid compound it forms, the motion which previously constituted it a gas, it takes up additional motion.'

Thus the form of matter which above all others would seem to possess the necessary requisites for the abundant occurrence of secondary redistributions, is living matter—in which there is embodied an enormous amount of potential and actual motion, whilst it, at the same time, possesses a degree of cohesion that permits temporary fixity of arrangement.

And accordingly, as Mr. Spencer says, 'The clearest, most numerous, and most varied illustrations of the advance in multiformity that accompanies the advance in integration, are furnished by living organic bodies.' He then adds:—'Distinguished as we found these to

be by the great quantity of their contained motion, they exhibit in an extreme degree the secondary redistributions which contained motion facilitates. The history of every plant and every animal, while it is a history of increasing bulk, is also a history of simultaneously-increasing differences among the parts. This transformation has several aspects. . . . The chemical composition which is almost uniform throughout the substance of a germ, vegetal or animal, gradually ceases to be uniform. . . . Simultaneously there arise contrasts of minute structure[1].' These contrasts gra-

[1] Loc. cit., p. 334. But these gradually increasing complexities of structure which are observable in the development of living matter, are only very typical instances of changes which perpetually tend to occur—although less obviously—in all other forms of matter. Masses of similar units, constantly acted upon by intrinsic and extrinsic forces, ever tend to become heterogeneous. Homogeneity, as Mr. Spencer has so fully explained, is a condition of unstable equilibrium. He says (loc. cit., p. 429):—'But all finite forms of the homogeneous—all forms of it which we can know or conceive, must inevitably lapse into heterogeneity. In three several ways does the persistence of force necessitate this. Setting external agencies aside, each unit of a homogeneous whole must be differently affected from any of the rest by the aggregate action of the rest upon it. The resultant forces exercised by the aggregate on each unit, being in no two cases alike in both amount and direction, and usually not in either, any incident force, even if uniform in amount and direction, cannot produce like effects on the units. And the various positions of the parts in relation to any incident force preventing them from receiving it in uniform amounts and directions, a further difference in the effects wrought on them is inevitably produced.' A very interesting example of such a differentiation of a homogeneous material may be seen by pouring a small quantity of varnish, composed of shell-lac dissolved in naphtha, upon paper (loc. cit., p. 404)—though the effects are here complicated by the rapid evaporation of the solvent.

dually increase, and different 'organs' slowly appear, varying in structure and arrangement in accordance with the nature of the organism whose evolution is being watched; so that, as Von Baer[1] originally pointed out, a progress from the more general to the more special is always to be observed during the development of animals and plants.

It is, therefore, only natural to suppose that homogeneous specks of living matter as they increase in size should undergo certain differentiating changes whereby 'structure' is gradually evolved and more and more complex functions are generated[2]. We should, however, have been utterly unable to predict the probable nature of such changes if we knew nothing of present lower organisms, and of the microscopic anatomy of the tissues of higher animals and plants during the different phases of their development. But our familiarity with these subjects has instructed us as to

[1] In his celebrated work entitled 'Ueber Entwickelungs-Geschichte,' 1828. The application of this law was subsequently developed by Dr. Martin Barry, in the 'Edin. Philos. Journal' for 1837, and also quite independently by M. Milne-Edwards in 'Ann. des Sc. Nat.' Sér. iii. t. i.

[2] Mr. Spencer says (loc. cit., p. 387):—'In Organisms the advance towards a more integrated, heterogeneous, and definite distribution of the retained *motion*, which accompanies the advance towards a more integrated, heterogeneous, and definite distribution of the component matter, is mainly what we understand as the development of functions. All active functions are either sensible movements, as those produced by contractile organs; or such insensible movements as those propagated through the nerves; or such insensible movements as those by which, in secreting organs, molecular rearrangements are effected, and new combinations of matter produced.'

the forms which minute specks of living matter tend to assume; just as the study of crystallography has taught us much concerning the limits of variation observable in crystalline forms.

The facts which are known concerning organic development in general, and embryology in particular—revealing, as they do, the before-mentioned progress from a minute homogeneous germ to the greater and greater complexity of structure peculiar to the various forms of life—are so many indubitable evidences of the tendency to develop which exists in living matter. As Mr. Spencer says[1]:—'Each organism exhibits, within a short space of time, a series of changes which, when supposed to occupy a period indefinitely great, and to go on in various ways instead of one way, give us a tolerably clear conception of organic evolution in general.' Nay, more, in the development of the individual we have a condensed embodiment of the modifications which have slowly appeared in one or other of the various representatives of an innumerable series of more and more specialized ancestors.

All Evolutionists, therefore, might be presumed to believe that the persistent intrinsic mutability of living matter, subject as it is to the constant incidence of ever-varying physical forces, would cause it to manifest a continuous tendency to undergo a further differentiation of structure[2]. Whilst all our knowledge

[1] 'Principles of Biology,' vol. i. p. 349.

[2] For as Mr. Spencer points out ('First Principles,' 2nd ed. p. 548):

of the multitudinous forms of living matter in existence impels us to believe (however difficult it may be to understand) that this mere inherent tendency to undergo differentiation, whose existence we are able to affirm *à priori*, is identical with, or at least displays itself as, what we are in the habit of calling an 'organizing' tendency.

Such is the conclusion which a survey of the facts seems almost to force upon us. And as it appears to me, we cannot consistently deny that living matter possesses an inherent tendency to develop, unless we are prepared at the same time to deny one of the first principles of the Evolution philosophy, by which we are taught that a homogeneous aggregate must inevitably become heterogeneous, and that the amount of heterogeneity must continually increase, partly through the operation of intrinsic attractions, and partly under the influence of externally incident forces. A new-born speck of living matter is merely a specimen of such a homogeneous aggregate which, from its very nature, is especially suited to undergo secondary differentiations. That such differentiations would be identical with 'organizing' changes could not have been predicated *à priori*; although, after observation has taught

'Every differentiated part is not simply a seat of further differentiations, but also a parent of further differentiations; since in growing unlike other parts, it becomes a centre of unlike reactions on incident forces, and by so adding to the diversity of forces at work, adds to the diversity of effects produced. This multiplication of effects is proved to be similarly traceable throughout Nature.'

us that this is so, we become entitled to affirm that the inherent tendency to differentiation possessed by living matter is an inherent tendency to become 'organized,' however much these processes of organization may be influenced, and in part determined, by the concurrent operation of varying extrinsic forces[1].

Just as surely, therefore, as the form of any particular Crystal is the result of the polarities of its aggregating molecules (inherent tendencies) under the influence of the conditions amidst which aggregation takes place; so is the more slowly-evolved form and structure of the Organism attributable to the inherent tendencies (molecular polarities) of its component matter and the forces operative upon it[2]. Changes in the external forces or 'conditions' may in each case produce more or less variation in the form which presents

[1] This affirmation of the existence of an inherent tendency in living matter to become organized has frequently been made, by De Maillet, Dr. Erasmus Darwin, Lamarck and others—although different writers have varied much in the amount of influence which they have been inclined to attribute to it. (See Spencer's 'Principles of Biology,' vol. i. pp. 402–410.) Some, indeed, have thought that the tendency had a natural, whilst others have referred it to a supernatural origin, and have considered (like Professor Owen) that it took effect under the supervision or predestination of an intelligent designer. The existence of the tendency has, however, been altogether denied by Mr. Herbert Spencer (loc. cit., p. 430), although we venture to think that this denial is inconsistent with the general principles of Evolution which he has so admirably enunciated.

[2] According to Müller, a doctrine somewhat similar to this seems to have been put forth by Reil nearly forty years ago, in the 'Archiv für Physiologie,' Bd. I.

itself; but it is impossible for us to assume that these effects have been induced by such mere modifying influences. Inherent attractions would of themselves tend to make a homogeneous aggregate become heterogeneous; and 'inherent tendencies' are not annulled but merely modified by the incidence of new external forces, which produce effects only when they are capable of inducing some alteration in the molecular arrangement of the matter itself. Thus it is that the persistent mutability of 'living' matter is *the* essential cause of the marked tendency which it manifests to become more and more differentiated (or organized); though in this respect 'living' matter only displays, in a highly marked degree, a property which is more or less possessed by all forms of matter—the homogeneous ever tending to become heterogeneous[1].

New-born living matter must, therefore, inevitably undergo differentiation. And just as this differentiation in all past time has gone along those grooves which result in the appearance of what we call 'organization,' so is present new-born living matter likely

[1] As long as Archebiosis or Heterogenesis were disbelieved in as present or recent occurrences, the occurrence of the very simplest forms of life in the present day seemed quite incompatible with the existence in living matter of an 'inherent tendency' to develop. Hence it may be that Mr. Spencer was led to deny, in its applicability to this particular case, one of the first principles of the Evolution philosophy. A belief in the present and continual occurrence of Archebiosis, however, would relieve him from all difficulties.

to go through essentially similar changes. Again, since nobody can know how rapidly the various lower 'specific' forms were evolved upon the surface of the earth in past times—and even in all subsequent periods preceding our own—the experimental evidence and the knowledge derived from actual observation which we now possess concerning the present comparatively rapid appearance of specific forms, must be regarded as so many contributions to our positive knowledge upon the subject, which far outweigh in value all the *à priori* deductions of even the profoundest reasoners.

Observation and experiment combined reveal the fact that new-born specks of living matter, after emerging into the region of the visible, continue to increase in size and soon exhibit signs of a primary differentiation. But the rapidity with which any degree of complexity of organization manifests itself will be found to vary very much with the mode of nutrition of the living aggregate, and with its manner of growth. The lower Fungi, Algæ, and Lichens are produced in great part by a mere 'irrelative repetition' of similar parts—though their growth may be either continuous or discontinuous[1]. But the nutrition of all these lower forms of life is effected by a synthesis of new living matter from not-living elements, and they further agree amongst themselves in manifesting a comparatively slow rate of variation. On the other hand, in Amœbæ, Flagellated Monads, Ciliated Infu-

[1] See note, p. 92.

soria, and the more animal forms of Protists—where actual solid organic matter is variously taken into the substance of the body and assimilated in the form of food[1], and where the organism constitutes more of an individualized whole—the transformative organizing changes are often much more sudden and striking.

[1] On this subject Professor Owen says ('Anat. of the Vertebrates,' vol. iii. 1868, p. 818):—'Amber or steel when magnetized seem to exercise "selection:" they do not attract all substances alike. To the suitable ones at due distance they tend to move; but, through density of constitution, cannot outstretch thereto; so they draw the attracted substance to themselves. If the amber be not rubbed, or the steel bar otherwise magnetised, they are "dead" to such power. The movement of a free body to a magnet has always excited interest, often wonder, from its analogy to the self-motion so common and apparently peculiar to "life." . . . A speck of protogenal jelly or of sarcode, if alive, shows analogous relations to certain substances: but the soft-yielding tissue allows the part next the attractive matter to move thereto, and then by retraction to draw such matter into the sarcodal mass, which overspreads, dissolves, and assimilates it. We say that the *Protogenes* or *Amœba* has extended a "pseudopod," has seized its prey, has drawn it in, swallowed, and digested it. No "organs," however, are recognizable; neither muscle, mouth, nor stomach. . . . If the portion of iron attracted by the magnet became blended with the substance of its attractor, the analogy thereto of the act of the *Amœba* would be, perhaps, closer, more just, than that other analogy which is expressed by terms borrowed from the procedure of higher organisms. . . . From certain knowledge of the homogeneous, by some termed "unorganized," texture of *Protogenes* and *Amœba*, we cannot predicate of their having sensation or exercising volition. Given "life" and suitable organic substances at due distances, the act of making contact seems as inevitable, as independent of any volition of the *Amœba*, as in the case of amber or steel, given "magnetisation," and attractable substances at due distances.'

The portions of organic matter which the Amœba or the Ciliated Infusorium absorb, slowly undergo, in the body of the organism, a process of molecular disintegration. The changes which the matter passes through in this situation and under such influences are, however, quite different from those which the same matter might have undergone if it had disintegrated in the water outside the organism. The formation of binary and ternary combinations, accompanied by the emission of some of these as gaseous products, does not occur when the matter disintegrates under the immediate influence of the molecular movements of the living matter by which it is surrounded. Ordinary molecular or chemical tendencies are conquered by more potent influences, just as the tendency of heavy masses to gravitate may be annulled by the occurrence of rapid axial rotations—such as we are familiar with in the case of the gyroscope. And similarly, the mode in which the movement of a rotating body of this kind may gradually communicate itself to a motionless ring with which its poles are connected, affords a suggestive illustration of the coercive nature or tendency of pre-existing molecular movements, by virtue of which each kind of living matter is enabled to grow after its own likeness[1]. Facts of this nature,

[1] Mr. Spencer says:—'Already, when treating of the nutrition of parts (§ 64), it was pointed out that we are obliged to recognize a power possessed by each tissue to build up out of the materials brought to it, molecules of the same type as those of which it is formed. This

as well as others, have been lost sight of by persons who believe that ordinary chemical processes belong to a different category from those which go on during the growth of living things. The difference exists, but it is merely one of degree, as we have previously endeavoured to show[1].

After having made these general preliminary remarks, we must now endeavour to ascertain the nature

building up of like molecules seems explicable as caused by the tendency of the new components which the blood supplies, to acquire movements isochronous with those of the like components of the tissue; which they can do only by uniting into like compound molecules.' ('Principles of Biology,' vol. ii. p. 349.)

[1] Many of the older physiologists, including Johannes Müller (see 'Physiology,' translation by Baly, 1840, p. 4), taught that there was not only an antagonism but a fundamental difference between ordinary chemical phenomena and those which go on in living things. The natural tendency which exists for some forms of matter to fall into more and more complex states of combination was not sufficiently considered. Mr. Hinton in his 'Life in Nature' also dwells much upon the supposed radical difference. He constantly speaks of 'vital force' as being 'opposed to chemical force,' instead of being merely the result of another order of chemical affinities. But yet his views concerning 'vital force' were in other respects quite in accordance with those professed in this work. He says (loc. cit., p. 68):—We perceive that from our present point of view the vital force exists simply in a peculiar arrangement of elements, involving a tension of a special kind. By whatsoever means this arrangement may be produced, the force thus embodied in it is equally called vital. The characters of the force are due to that arrangement; they flow from it rather than are concerned in its production; just as in the case of the other forces, such as heat or electricity, the peculiar properties they manifest are the results and not the causes of the states of matter in which they consist.' Another statement to the same effect is made at p. 155.

of the early developmental forms which our new-born specks of living matter have been found to assume; and also seek to unravel the mutual relations that exist between these several forms.

Much doubt and uncertainty have always prevailed in the minds of naturalists with reference to the nature of *Bacteria*, and the degree and kind of relationship which they present to the *Mucedineæ* and other low forms of Fungi.

By some naturalists and pathologists, *Bacteria* and *Vibriones* are regarded as distinct and independent species, having no developmental outcome in higher forms, and no connection with the life-history of Fungi. This was the old view, and strangely enough such a notion has been advocated again, even quite recently[1]. Others (not believing in the occurrence of Archebiosis) who have traced some of the ultimate developments of *Bacteria*, are inclined to regard them as necessary links or stages in the life-history of many Fungi. Whilst a third party, accepting many of the facts of development just alluded to, and believing that some *Bacteria* and *Vibriones* do develop into Fungi, maintain that these primary forms may arise *de novo*, and that they are therefore not necessarily derived from pre-existing Fungi.

When we find Dr. Sanderson[2] adducing certain

[1] See Dr. Sanderson's memoir in 'Thirteenth Report of the Medical Officer of the Privy Council,' pp. 48 and 68.

[2] Loc. cit., p. 68.

'proofs' that 'fungi are not developed from microzymes' (*Bacteria*), of course no reference is made to their direct development, through *Vibrio* and *Lepothrix* forms, into a fungus mycelium. Yet this mode of development has been seen by Professor Hallier and others as well as by myself. Dr. Sanderson's remarks have reference to the views recently put forth by Professor Huxley with regard to the relations of *Bacteria* to *Torulæ*[1]. The observations on which they were founded are, however, by no means extensive enough to give warrant to the belief that 'fungi are not developed from microzymes;' and such a view is contra-indicated by the observations of others[2].

We agree, however, with Professor Hallier in many respects. We believe with him that mere particles of

[1] 'Quarterly Journal of Microscopical Society,' Oct. 1870. In his memoir Professor Sanderson more especially lays stress upon the fact that *Bacteria* may appear in liquids without the presence of *Torulæ*; but I had previously ('Modes of Origin of Lowest Organisms,' 1871, p. 10), not only called attention to this fact, but to the still stronger one of the occurrence of *Torulæ* in fluids in which not a single *Bacterium* was to be found. Further experience has only served to strengthen my opinion as to the untenability of Professor Huxley's view; see also vol. i. p. 8.

[2] Neither does Professor Sanderson's conclusion as to the probable derivation of some of the *Torulæ* and fungi which were found in his experiments seem to me satisfactory. Unless he can prove, what no one else has been able to do, and what is directly opposed to much other evidence—viz. that *Torula* germs are not destroyed by boiling water—he has no sufficient warrant, in the face of the experiments of others, for resorting to the belief that such organisms have been derived from *invisible* germs existing either in air or water (loc. cit., pp. 69 and 56).

living matter ('micrococci') may develop under different conditions—and especially in different media—either into *Torulæ* ('cryptococci'), or into *Bacteria* ('arthrococci'), and that both these forms may subsequently grow into perfect Fungi. We do not agree with him, however, in his view that 'micrococci' are invariably derived from fungi, and that they constitute some of the normal reproductive units of these organisms. We believe, and we think we have proved, that *Bacteria* are frequently evolved *de novo*; and we further believe that when they do arise from pre-existing fungi, they are either thrown off accidentally as mere living particles, or on the other hand that they result from the heterogenetic breaking up of the protoplasmic contents of a previously normal fungus-spore or sporangium[1]. So that when derived from pre-existing fungi in any stage of development, they are accidental and occasional, rather than normal and constant products.

We believe that *Bacteria* and *Torulæ* merely represent two of the most prevalent forms which specks of new-born living matter are prone to assume; and in confirmation of this view, we may state that all intermediate shapes are frequently to be seen between the most typical *Bacteria* and the most typical *Torulæ*; just as all intermediate conditions are to be seen between the smallest *Bacteria* of some highly fermentable infusion,

[1] Just as *Bacteria* may result from retrogressive changes taking place in an *Amœba* which has reached the term of its existence (see p. 222).

and the larger *Vibrio*-like forms which are frequently met with in almost similar fluids.

The opinion seems to be pretty common that *Vibriones* are higher organisms than *Bacteria*, and that *Torulæ* are higher than either. Both these views, however, must be received with certain qualifications.

It is well known that the more slowly crystalline matter separates from a solution, the smaller is the number and the greater the size and perfection of the crystals which appear. Mere amorphous granules are deposited in many cases when the separation takes place instantaneously. And, similarly, I have found on several occasions that the most fermentable solutions swarm rapidly with inconceivable numbers of small *Bacteria*; though if a drop of acetic acid has been added to another portion of the same infusion, it does not become turbid till many hours later, and the *Bacteria* which are present in the first scum may be much larger and such as are generally termed *Vibriones*. In other cases, highly fermentable fluids which have been subjected to the influence of very high temperatures (270°F and upwards) will, even when exposed to the air, yield neither *Bacteria* nor *Vibriones*—though *Torulæ* appear after a time, and multiply more slowly.

Now with reference to such observations, the following considerations must be borne in mind. A highly fermentable solution is in one respect exactly comparable with a supersaturated saline solution. Both contain chemical elements which have a strong

tendency to combine, and in both cases the products of combination are insoluble—particles of crystalline matter appear in the one case, and particles of living matter in the other. But the living matter differs essentially from the crystalline matter by reason of the complexity of its constituent molecules, and their constant tendency to undergo small variations in composition or minute relative rearrangements. This capacity for intestine molecular movement, which is one of the most distinctive attributes of living matter, might well be most marked in the products which separate from the most fermentable solutions; and it is precisely this attribute which is the principal factor in bringing about the self-multiplication, or discontinuous growth, of living units.

Thus it is that rapid growth and rapid fission frequently go on simultaneously; so that although the total amount of living matter which separates from the solution may be large, the individual living units are very small. 'Discontinuous' growth is in excess, and therefore the fact of the growth being really rapid is apt to be overlooked.

All the differences in size and form recognizable between small *Bacteria* and large *Bacteria*; between the latter and *Vibriones*, whether jointed or unjointed; between *Vibriones* and *Leptothrix* filaments, plain or segmented in various ways; and between *Leptothrix* and mycelial filaments of a *Fungus*, are easily explicable in accordance with these considerations. The several

forms, many of which frequently occur together in the same solution, depend upon the frequency with which

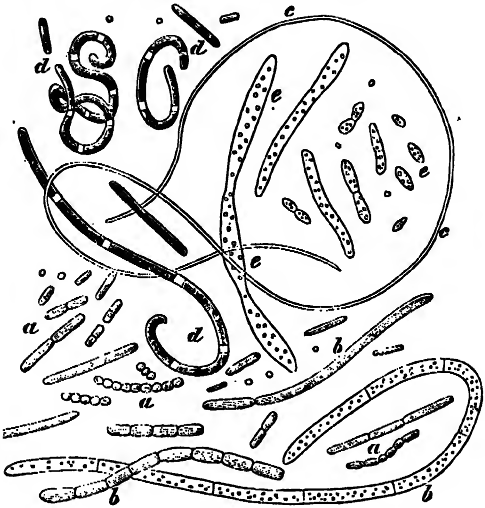


FIG. 46.

Bacteria growing into *Vibriones, Leptothrix*, and *Spirillum*. (× 1670.)

a, a. Different kinds of *Bacteria* and *Vibriones*.
b, b, and *c*. Different kinds of *Leptothrix* filaments.
d, d. Rudimentary *Spirilla*, some of which were ultimately seen to give rise to Fungus-mycelia.
e, e. Torula-like *Bacteria* developing into Fungus-mycelia.

segmentation tends to occur, and upon the degree of completeness of the process [1]. Where the segmentation

[1] Speaking of these various forms, M. Ch. Robin says:—'Les observateurs modernes les plus experimentés n'ont pas encore pu trouver des caractères spécifiques et distinctifs réels, permettant de séparer en plusieurs espèces les corps organisés très-repandues designés ici, bien que des vues théoriques ou des observations trop restreintes aient fait croire le contraire.' ('Traité du Microscope,' 1871, p. 929.)

is incomplete the dissepimented parts have a tendency to grow in unison, so that an actual increase in size of the several parts and of the whole organism takes place. But where the segmentation is complete and rapid, no increase in bulk of the respective units is able to occur.

As growth progresses it is almost invariably found, on examination with high microscopic powers, that the *Bacteria*, which at first appear to have a homogeneous consistence, gradually acquire a hollow character. They seem to undergo the first differentiation, which results in the separation of an outer and more consistent layer from an internal and more fluid contents. The same kind of thing is visible in *Vibriones* and *Leptothrix* filaments—though in them, and also in Fungus-mycelium of different kinds, the extent to which this differentiation is perceptible is very various. Some specimens of each of these filaments seem more or less semi-solid throughout, and present no distinct bounding wall—whilst in others the bounding wall is most apparent, and no solid contents are recognizable. The size of the segments, or the frequency with which dissepiments occur, is as variable in *Vibriones* and *Leptothrix* as in mycelial filaments.

We see in some *Vibriones* and *Leptothrix* filaments that the segments are short and deeply divided, so as to produce necklace-like chains, each unit of which more closely resembles the *Torula*-corpuscle than a *Bacterium*.

The forms of *Torulæ*, however, are almost infinite in variety as they occur in different situations, and they are often notably different even in the same solution. They may vary in size from the minutest visible speck to a vesicle $\frac{1}{2000}''$ or more in diameter. They may after

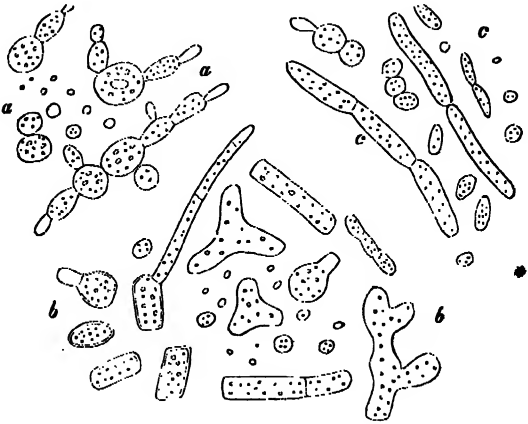


FIG. 47.

Different Forms of *Torulæ*. (Turpin [1].)

a. Forms from beer, growing in sugar and water (*Torula cerevisea*).
b. Forms from beer-wort—early stages of *Mycoderma cerevisea*.
c. Forms from filtered apple-juice.

a time become spherical, ovoidal, ellipsoidal, or cylindrical in form. They may be motionless or mobile[2],

[1] Selected from Plates 3, 4, and 5 of his Paper in 'Mém. de l'Acad. Roy. de France,' 1846.

[2] *Torula* corpuscles may often be observed to exhibit pretty brisk oscillations when they are small, though the movements generally cease after the corpuscles have attained a certain size. These movements are obviously not 'Brownian' in character.

colourless or coloured[1]. They may be almost naked masses of protoplasm, or they may present a bounding membrane of various degrees of thickness. They may be nucleolated or non-nucleolated; provided with vacuoles or devoid of vacuoles; so that oftentimes they exist as mere minute spherical or elongated vesicles,

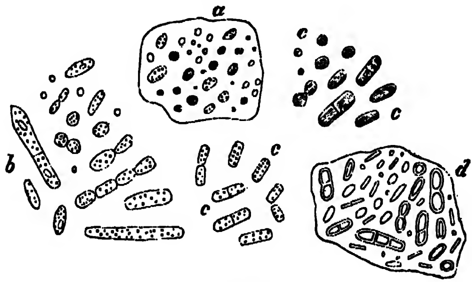


FIG. 48.

Forms illustrating interchangeability of *Torulæ* and *Bacteria*. (× 1670.)

a. Minute *Torulæ* growing in jelly after the fashion of *Bacteria*, from hay infusion.
b. *Bacteria* with *Torula*-like forms, from beef infusion.
c. Homogeneous *Bacteria*, more or less like *Torulæ* in form.
d. Fungus-spores developing in a homogeneous film—many of them in their early stages having the shape of *Bacteria*—from the surface of an old hay infusion.

composed of a minutely-granular protoplasm slightly condensed at the surface. In this condition no dif-

[1] Either brown or green. All transitions may be seen from the colourless condition to the brown tint which is so frequently assumed by fungus-spores (p. 233), and similarly all transitions may be seen from the colourless to the green tint (vol. i. pp. 364, 450). The new-born specks of matter which become green tend, however, to develop into Algæ rather than Fungi.

ference can be detected between them and some forms of *Bacteria*, so that many units are to be met with in various infusions to which either name might be applied with equal justice. *Torula*-forms appear and multiply, moreover, in the midst of homogeneous gelatinous films or in the midst of jelly masses, just as obviously (if not quite so frequently) as *Bacteria*-forms, which, when they grow in the latter manner, often constitute masses known by the name of *Zooglæa*.

It is impossible for us to assign any ultimate reason why one rather than the other of these forms should manifest itself. We can only observe that in some solutions *Bacteria* most frequently present themselves, whilst in others *Torulæ* are most prone to occur. It has been known, for instance, since the time of Dutrochet that the organic forms met with in acid and alkaline or neutral solutions vary; and it has been frequently observed by others, that *Torulæ* are most apt to present themselves in slightly acid solutions. Again, whilst the most putrescible solutions almost invariably yield *Bacteria*, the same fluids, after their fermentability has been impaired by the influence of heat, may engender nothing but *Torulæ*[1]. *Torulæ* are generally

[1] M. Pouchet frequently insisted upon the fact that exposure of the same fluid to higher atmospheric temperatures (by increasing the fermentability of the fluid) rendered it most prone to yield *Bacteria*, although at lower temperatures it would yield *Torulæ* and Fungi ('Nouv. Expér.' p. 179). And, similarly, when *Torulæ* are in the habit of vegetating into Fungi at lower temperatures, 'si la température est

more frequent in saline solutions than *Bacteria*, and in some of these after they have been boiled no *Bacteria* ever present themselves. This, for instance, has been found to be the case with ammonic tartrate and sodic phosphate solutions; although when the former salt has been replaced by ammonic carbonate, such a solution has yielded *Bacteria* after it has been boiled[1].

Because in some solutions *Bacteria* reproduce *Bacteria*, and in other solutions *Torulæ* reproduce *Torulæ*—because each of these forms 'breeds true'—they have been regarded by many as distinct 'species.' This, however, is not altogether conclusive. A fragment which detaches itself from one of the lowest living things has just as much tendency to grow into the form of its parent, as the fragment detached from a given crystal has to reproduce a similar crystalline form. In each case, however, the parent form is reproduced only so long as the conditions remain the same. Placed under new conditions the crystalline fragment *may* grow up with a modified form, and, similarly, a change may overtake a portion of matter thrown off from a pre-existing living form. In order that the crystal may lapse into another form, it seems necessary that the new conditions shall be capable of bringing about a new molecular arrangement (or allotropic state) of the

trop élevée, la germination se fait d'une manière confuse ou ne se produit pas; les spores spontanées s'altèrent avant qu'elle ait lieu, et le liquide se remplit de Bacteriums, de Monades, ou de Vibrions.' (Loc. cit., p. 175, note 1.)

[1] Compare *Exps. n* and *x* with *Exps. y* and *z* (vol. i. p. 462).

crystalline matter. And, similarly, new conditions probably operate upon living forms, only so long as they are capable of inducing new molecular combinations and modes of activity. It is therefore to be expected that new-born organic forms should remain constant so long as we have to do with the same fluids under unaltered conditions.

Since it is well known that *Bacteria* and *Torulæ* may frequently be seen to grow in the same solution, we are compelled to believe that some minute difference in the constitution of their ultimate units does exist, and that each has the power of causing, during its acts of growth, the synthesis of similar units of living matter. The occurrence of one or of the other form is, therefore, not always nor wholly attributable to mere difference of 'conditions'—it must be mostly due to an actual, though minute, difference in the molecular constitution of the initial units of living matter. For although the same crystalline matter under the influence of different conditions may assume different crystalline forms, it is much more common for different crystallizable compounds to aggregate into different geometrical forms.

It must, moreover, be quite familiar to all who have had much experience in this particular line of research, that *Torulæ* frequently exist in abundance in certain solutions, and yet show no signs of developing into Fungi. Discontinuous growth goes on rather than continuous growth. So much is this the case, that it

was not until 1840 that the development of *Torulæ* into Fungi was traced[1]. The multiplication of *Bacteria*, however, even more frequently takes place without much tendency to the evolution of higher forms; and when this does occur it is not quite so easily recognizable as the development of *Torulæ*. And yet in those cases where the conditions are, as we must suppose, favourable to such continuous modes of growth, certain *Bacteria* may enlarge into *Vibrio*-forms, these into *Leptothrix*, and the latter into larger and more definite fungus-mycelia, just as surely and just as readily as the *Torula* corpuscle buds out into a growing fungus-mycelium[2].

The stages by which a *Torula* corpuscle develops

[1] It was believed by Kützing to be one of the unicellular Algæ, and the same view was afterwards adopted by Robin, in spite of the statements of Turpin. The development of *Torulæ* at once into fungus-mycelia, producing the mother of vinegar (*Mycoderma aceta*), contrasts notably with the discontinuous mode of growth and perpetuation of the *Torula* form, which obtains in some forms of the vinous fermentation.

[2] I have found the former development take place very readily in certain infusions to which a minute fragment of cheese had been added, and also in some solutions of ammonic tartrate and sodic phosphate. (See *Appendix* D, Exps. xvii, xix, and lii). In other cases *Vibrio*-forms may assume the *Spirillum* mode of growth, rather than that of *Leptothrix*, before giving origin to a fungus-mycelium—as I have seen in an infusion containing young twigs of the common elder. (*Fig.* 47 *d.*) Between *Leptothrix* again and some of the colourless *Oscillatoriæ* there seems to be no real ascertainable distinction. Moreover, all shades of increasing greenness exist amongst the representatives of this latter family, which by common consent is included amongst the Algæ; so that they constitute transitions between the simplest Fungi and Algæ.

into one of the simpler fungi, with its various kinds of reproductive units—conidia or spores—is most simple,

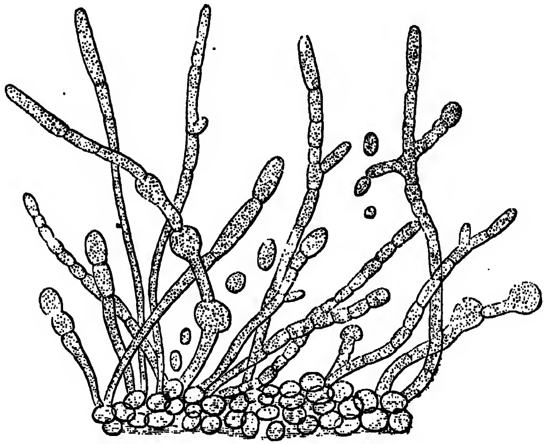


FIG. 49.

Development of *Torulæ* found in Cider. (Pouchet.)

Illustrates the irregular manner in which Segmentation of the Filaments occurs, their irregularities in size, and the separation of 'conidia.'

and has been well described by M. Trécul[1]. The means he adopted to observe the development, also tend to substantiate what I have already said with reference to the cause of continuous as opposed to discontinuous growth, and the association of the latter process with higher degrees of fermentability. In order to bring

[1] 'Conidia' is the name applied to the simplest kind of spore—to those which are separated from any portion of the mycelium by a simple process of strangulation (fission), as in *Fig.* 49.

about the development of the *Torulæ* which multiply as such in beer-wort, M. Trécul has been in the habit of pouring away the supernatant beer from some of the dregs, and replacing it with water. A portion of this mixture placed on a slip and protected by a covering glass, may then be kept in a damp chamber and watched from time to time under the microscope.

Some of the corpuscles develop large vacuoles in their interior, whilst in others in which the plasma is thin, refractive globules of various sizes are produced within the cell[1]. These globules on the rupture of the parent cell are capable of enlarging and giving rise to new *Torula* corpuscles.

The corpuscles always vary much in shape—some are spherical and others ellipsoidal, whilst others again are more or less cylindrical. In their growth the two former kinds often tend to pass into the cylindrical variety, and then they segment occasionally after the fashion of *Bacteria*[2]. Other spherical or ovoidal cells may protrude a process from one extremity which is much narrower than the cell itself; and this, as it grows into a short filament, may give off still smaller lateral filaments. When the germinating cells are only a little longer than they are broad, a filament often grows out from one side (near the extremity), whilst another

[1] The number of these globules varies very much. Where the plasma is very thin they are scarce, but in better nourished cells they may be quite tightly packed.

[2] See *Fig.* 47.

may issue near the opposite extremity, either from the same or from the other side. Then one or other of various modes of development may occur:—

a. The filament may expand into a long or short single cell, and then may segment into conidia, which, after a little delay, themselves undergo a process of germination.

b. Or the filament may continue to grow, forming dissepiments, or undergoing a partial process of division at intervals. The terminal division of such a filament may then divide (as in the last case) into globular or ellipsoidal conidia. Many of the filaments may in addition send out lateral prolongations, and some of the shorter of them may also break up into conidia.

c. The changes already mentioned are to be seen in those corpuscles which germinate beneath the covering glass, but where the process occurs outside the edge of the glass, the resulting growth is much more luxuriant. The larger filaments develop branches of the second, third, and fourth order—all being made up of oblong cells produced by partial segmentation. And any of the terminal ramifications may break up in various ways into reproductive units or conidia. Before this process occurs, M. Trécul says: 'The extremities of the filaments ordinarily assume an indistinct but bright and highly refractive aspect, similar to what the conidia themselves preserve after the process of segmentation.' Where the filaments remain in the recumbent position, they may develop either one or two series of conidia;

whilst those which grow upwards often give rise to several chaplet-like series of these reproductive corpuscles[1].

Thus by the mere repetition of similar phenomena—in which a process of partial segmentation is largely concerned—a comparatively complex fungus-growth (*Penicillium*) results, differing only in minor respects from those which have been found in some of my solutions taken from closed and superheated flasks.

The particular forms assumed by the outgrowths from the germinating corpuscles, seem subject to much natural variation for which we are utterly unable to account. M. Trécul found that on exposure to the air the cells of beer yeast grew partly into the form of *Mycoderma cerevisiæ*, and partly into that of a large *Penicillium*. He has become convinced, therefore, that the view originally advocated by Turpin is correct, viz. that *Mycoderma* and *Penicillium* are simply two forms which may be assumed by germinating beer *Torulæ*[2]. Nay, more, the *Mycoderma* itself is observed to be most changeable in its form, as the qualities of the fluid in which it grows alter; and an already growing *Mycoderma* is said to be capable of taking on the mode of growth characteristic of *Penicillium*[3].

[1] See *Fig.* 53. This mode of development into forms resembling *Penicillium glaucum*, was observed by Turpin in 1840, by the Rev. M. J. Berkeley in 1855 (from porter yeast), and subsequently, by M. Pouchet, in beer yeast and in that from cider.

[2] See also 'Compt. Rend.' t. 67, p. 1164.

[3] On the other hand the relationship existing between *Mycoderma* and *Torulæ* is most distinct. When beer-wort is exposed to the open air

M. Pouchet's observations on the development of the *Torulæ* which appear in cider had previously led him to express almost similar opinions with regard to the convertibility of the several forms of the *Mucedineæ*, which are apt to appear in such a solution. He says:—'Le cidre que nous avons si longuement étudié, nous offre un assez grand nombre de formes végétales. Mais presque toutes les espèces appartiennent au genre *Penicillium*; d'autres, en moindre nombres aux *Aspergillus*.' There are two principal forms of *Penicillium*—the submerged and the aerial—the former constituting a group which M. Pouchet was the first to describe. Several varieties exist, both of the submerged and of the aerial forms; and when the temperature is

and is not disturbed, *Mycoderma* begins to develop in about forty-eight hours—though curiously enough its appearance may be delayed for a fortnight or more by agitating the liquid two or three times daily. It commences in the form of the minutest specks, which gradually enlarge into ellipsoid corpuscles; these give birth after a time to a little bud at one extremity, and this grows into a corpuscle which in its turn produces another. Lateral buds are also produced, and after a time this mode of growth results in elegant, much-branched tufts, 'qui se modifient dans leur forme à mesure que l'alteration du liquide avance.' But when beer-wort containing the *Mycoderma* is poured into a bottle (so as to fill it) which is then tightly stoppered, the plant ceases to grow in this form and gives place to an abundance of *Torulæ*—these being partly derived from portions of the pre-existing *Mycoderma* and partly the results of a new formation. The *Torula* form, and 'discontinuous' mode of growth, is that which seems to be invariably engendered when the liquid becomes more or less charged with CO_2 and alcohol, and when the pressure increases (see vol. i. p. 420). Boiled beer-wort in a sealed vessel also produces *Torulæ* where there has been no pre-existing *Mycoderma*; and, according to M. Trécul, the *Torulæ*, thus engendered, after exposure to the air will also gradually assume the form of *Mycoderma*.

low, and the cider weak, a growth belonging to another generic type is very apt to occur, which M. Pouchet named *Aspergillus polymorphus*, on account of the extreme

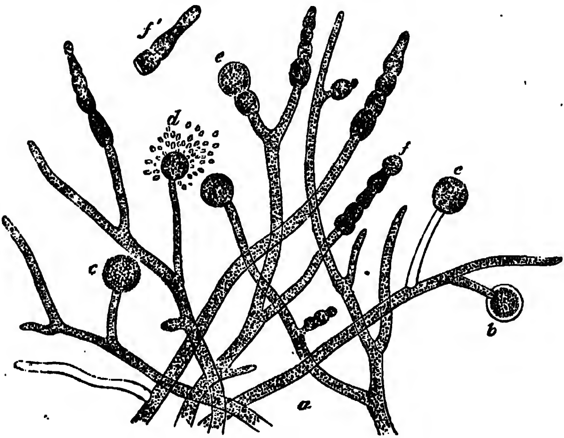


FIG. 50.

Another Fungus (*A. polymorphus*) found in Cider. (Pouchet.)

Showing other modes by which the terminations of the filaments give rise to spores or 'conidia.'

diversity in the form of its terminal reproductive aggregations [1].

[1] M. Pouchet says:—'Un fait extrêmement remarquable chez *l'Aspergillus* du cidre c'est que les spores spontanées d'ou sortent les plantes, ne ressemblent nullement à celles qui naissent sur les conceptacles. Les spores spontanées sont beaucoup plus volumineuses, et tombent au fond de la liqueur; tandis que les spores des conceptacles, considérablement plus petites, plus légères, viennent flotter à sa surface. Enfin on surprend en germination autant de spores spontanées qu'on le veut, tandis que jamais je n'ai vu germer une spore provenant de la plante.' ('Nouvelles Expériences,' p. 182.)

We have thus endeavoured to show that the *Bacterium* and the *Torula* corpuscle are only different modes of growth which may be assumed by new-born specks of living matter, and that the varieties of each kind of growth are both numerous and transitional. Each of these forms, under suitable conditions, may grow in a continuous rather than in a discontinuous fashion[1], producing variously branched and articulated growths, which at intervals are apt again to revert to the discontinuous mode of growth, so as to produce reproductive units—either in single file, as buds from a terminal expansion, or by fission of the contents of a terminal chamber. These, and many other simple variations in the mode of production of the reproductive units, variously combined with different sizes, modes of branching, articulation and segmentation of the filaments, etc., go to produce the innumerable simpler kinds of Fungi which, instead of being lineal descendants of similar mutable organisms that lived in a pre-Adamite world, are only different modes of growth which may be assumed by new-born living matter[2]. This notion was to a certain extent favoured by the Rev. M. J. Berkeley when, after alluding to some of the many startling metamorphoses which are to be observed amongst these most changeable organic forms, he says[3]:—'It would thus seem that the opinions of

[1] For the reasons already stated this process is most easily watched during the development of *Torula*.

[2] Concerning the mutability of Fungi, see *Appendix* D, p. lxxvi.

[3] In Lindley's 'Vegetable Kingdom,' 3rd ed. 1853, p. 35.

those, who have asserted that the species or genus of a Fungus depends upon the matrix by which it is nourished, are at least specious; especially if we take the above fact in connection with the experiments of Dutrochet, who obtained different genera of mouldiness at will, by employing different infusions.'

Representatives of various kinds of simpler Fungi are produced from different *Torula* with the greatest ease. Throughout all the stages of their development there is merely a modified repetition of the simple processes which are ever taking place amongst *Bacteria* and *Torulæ* during their more familiar 'discontinuous' growth. Experiment and observation alike compel us to believe that the new-born specks of living matter unfold into these various organic forms, under the combined influence of intrinsic tendencies and extrinsic agencies—just as the forms of the various kinds of crystalline matter are due to the particular atomic or molecular combinations of which they are composed, subject to the influence of the conditions amidst which aggregation takes place[1].

[1] As we have already pointed out, organic matter is much more easily destructible than saline matter by exposure to very high temperatures; it is worthy of observation, therefore, that the saline solutions used in my experiments (270°—307°F) have proved much more productive than the simple organic infusions. The latter, moreover, have proved less and less productive with each fresh increment of heat. Omitting *Exp. k*, in which no carbon was present, the remaining twenty-four experiments may be thus tabulated:—

Our experiments, however, afford positive evidence that new-born specks of living matter may assume other forms than those already referred to. In Experiment *b*, the simplest forms of *Amœbæ* were found, and also flagellated *Monads* in company with various kinds of *Torula* corpuscles. These, then, are also organic forms which are capable of resulting from the direct unfolding of some specks of new-born living matter. The acceptance of such a view is rendered much more easy than it would otherwise have been, by the following considerations:—

When enumerating the modes of variation observable amongst *Torulæ*, we might almost have added that some were flagellated and active, whilst others were non-flagellated and inactive. And, however fundamental such distinctions may seem to be, their importance must be diminished in the minds of those who know that two forms of Fungi, in other respects almost indistinguishable from one another, may have reproductive units which are dissimilar in this re-

Temp. 270°–275° F for 20 minutes	4 Organic Infusions	2 Productive. 2 Unproductive.
	2 Saline Solutions	1 Productive. 1 Doubtful.
Temp. 293° F for 5–20 minutes	3 Organic Infusions	1 Productive. 2 Unproductive.
	5 Saline Solutions	5 Productive.
Temp. 295°–307° F for 4 hours	4 Organic Infusions	1 Productive. 3 Unproductive.
	6 Saline Solutions	3 Productive. 3 Unproductive.

spect. As previously mentioned[1], *Leptomitus* produces motionless reproductive corpuscles, whilst the protoplasm in the almost similar terminal chambers of *Achlya* produce active spores ('zoospores'), which after a time become stationary and develop filaments similar to those from which they have been derived. The reproductive units of other Fungi (*Mycetozoa*) appear as Amœbæ, which after a time themselves take on the characters and mode of growth characteristic of a fungus. And finally, each of these forms (Amœba and Monad) which is thus related to the common form (Fungus), has been proved by many observers to be easily interchangeable with the other. Encysted Amœbæ as we have learned from Haeckel, and others, give birth to flagellated Monads or 'zoospores,' and these in their turn lapse easily into the more slowly-moving reptant Amœbæ. Similar transitions, moreover, from the active flagellated Monad, to the slowly-moving vacuolated Amœba, I have seen dozens of times, whilst watching these forms under high microscopic powers, in various infusions. I have also seen numbers of simple, motionless corpuscles, resembling some forms of *Torulæ*, gradually begin to take on amœboid characters and movements[2].

Facts of this kind, even independently of those which will be subsequently adduced[3], tend to reveal

[1] See vol. i. p. 182, note 1.
[2] See *Fig.* 55.
[3] In Chap. xvii.

such an amount of community in nature between Amœbæ, Monads, and the matter of a Fungus, as to make it much easier for us to believe what the experiments indicate, viz. that *Torula* corpuscles, the simplest *Amœbæ*, and tailed *Monads*, may all originate by a process of Archebiosis, and that a certain interchangeability exists between them—so that a corpuscle of a certain size may slowly expand into one or other of these organic forms, according as its internal molecular movements gradually assume different modes of action.

But we still have to allude to other primordial forms which have been met with in our experimental flasks,—I allude to the green-coloured Pediastreæ, and bodies resembling the simplest Desmids, which have been found in the solutions containing iron and ammonic citrate. In many respects their presence is a matter of the highest interest. In about 200 experiments with heated fluids in closed vessels, I have never found even a fragment of green protoplasm except on five occasions. Each time actual organisms were found more or less similar to one another, and in each case a salt of iron existed as one of the constituents of the solution[1]. As iron is one of the constituents of chlorophyll, this correspondence is as much in accordance with the *de novo* origin of such organisms, as it is

[1] See vol. i. pp. 364, 365, and pp. 448–454.

difficult to reconcile with the views of those who do not accept this legitimate interpretation.

Some of the bodies found were partly like unicellular Algæ, and partly like ordinary *Torulæ*. They exhibited only the faintest green tint, and yet the general character of the corpuscles was less like *Torulæ* than that of some forms of *Protococcus*. Associated with them also was a filament of an algoid character. We have already pointed out the very gradual nature of the transitions which exist between Fungi and Algæ, by means of the various forms of *Leptothrix* and *Oscillatoria* filaments, to say nothing of other intermediate forms, such as *Achlya*, *Saprolegnia*, and similar types. The transitions between the *Mucedineæ* and *Leptothrix* are as gradual and unbroken, as those which exist between *Leptothrix* and the colourless *Oscillatoriæ*. The latter develop a green colour in an equally gradual manner, and insensibly take on characters which affiliate them to the other filamentous Algæ. And now our experiments tend to show, definitely, that there is no radical difference between Fungi and Algæ, but that the evolution of the one or the other is regulated in part by the mere presence or absence of certain constituents. Where no iron is present new-born specks of living matter may develop into *Bacteria* or *Torulæ* and gradually unfold into fungus-forms; but if iron be present such new-born specks may incorporate this element, develop green protoplasm, and assume the form of *Protococcus*, with ten-

dencies which may enable it ultimately to unfold into one or other of the filamentous Algæ [1].

On the other hand it must not be forgotten, that however close the alliance may be between Fungi and Algæ, the relationship is perhaps even closer between Fungi and Lichens [2]. This is the opinion of the Rev. M. J. Berkeley, who has included both under the common designation 'Mycetales;' and Professor Lindley also said that Fungi and Lichens are 'so closely allied that it is often difficult to tell to which division some given species may belong.' Dr. Lauder Lindsay, moreover, states [3] that 'there is a large group, provisionally termed "Fungo-Lichens," which have the characters equally of Fungi and Lichens, and which it is at present impossible to assign preferentially or exclusively to either family.' Some of the septate and compound spores which I have found in ammonic tartrate and sodic phosphate solutions are almost precisely similar to some spores of West Greenland Lichens which are depicted in Dr. Lindsay's very interesting memoir [4]. But the relationship between Algæ and Lichens is just as close. According to Fries, indeed, Lichens are types of Algals born in the air,

[1] The interchangeability of the two modes of growth will be substantiated by further evidence in subsequent chapters.

[2] See *Appendix* D, p. lxxvi.

[3] 'Trans. of Linn. Soc.' vol. xxvii. (1871), p. 308.

[4] Compare also his Fig. 13–16 of Pl. 51 with my Fig. *d.* of *Appendix* A.

and interrupted in their development by the deficiency of water[1].

In *Experiment b* the bodies found were of the brightest green colour, and were almost precisely like representatives of the genus *Scenodesmus*, which is usually included amongst the Pediastreæ. But in other experiments the organisms seemed more closely to resemble the simple Desmids belonging to the type known as *Arthrodesmus*, and some of these exhibited tendencies to grow in a filiform manner.

The conflicting opinions of naturalists concerning the affinities and degree of relationship existing between Algæ, Pediastreæ, and Desmids, are sufficient to show the close alliance of these various forms. Pritchard says in his 'History of Infusoria' (p. 30):—'Mr. Ralfs followed Ehrenberg, Meneghini, and others in placing the Pediastreæ among Desmidiæ; but Corda, Nägeli, and Braun have separated the two as distinct tribes. Indeed Mr. Ralfs has modified his views since the publication of his monograph, and would treat the Pediastreæ as a sub-family of Des-

[1] Professor Lindley writes:—'Pulverulent Lichens are the first plants that clothe the bare rocks of newly-formed islands in the midst of the ocean, foliaceous Lichens follow these, and then Mosses and Liverworts. They are found upon trees, rocks, stones, bricks, pales, and similar places; and the same species seem to be found in many different parts of the world—thus the Lichens of North America differ little from those of Europe. They are met with in one place or other, from the equator to the pole, and from the sea shore to the limits of eternal snow.' ('Vegetable Kingdom,' p. 47.)

midiæ. Nägeli ("Einzell. Alg.") arranges them with the Palmellæ as a distinct group, and in this has the support of Braun ("Gen. Nova," p. 69).' In a later portion (p. 752) of the work he says:—'So far as we can judge, it is not yet determined whether they should remain united with the Palmellaceæ to which they have been referred by Nägeli, or, with some few other Algæ, form a distinct group near Palmellaceæ, and perhaps Volvocineæ. They cannot, we think, continue to be considered as belonging to the Desmidiaceæ.' All these discrepancies of opinion are not difficult to understand if we bear in mind the absence of any real grounds for distinguishing the several forms, and at the same time consider the difficulties of the old systematic writers who, in accordance with their theoretical notions, felt bound to conceive that specific, generic, and family distinctions existed—even though observation taught them that the several forms were related to one another in much the same way as the different patterns observable in a kaleidoscope. Now experiment comes in to demonstrate the fundamental kinship which exists between the several forms.

Let the reader compare the representations of the organisms which have been found in the twenty-four experiments in which the flasks were heated to temperatures ranging from 290—307°F, and he will then perceive that, so far as we have gone, the views enunciated in the present chapter (founded in part upon the

microscopical examination, and study of the development, of some of the primordial forms of life) are substantiated by the most rigorous experimentation. No other conclusion remains for us but that the several organisms are products of the direct developmental unfolding of new-born specks of living matter. And yet amongst these forms we see *Bacteria*, *Vibriones*, *Leptothrix*, and *Torulæ*; *Fungus* filaments with and without fructification; *Protamœbæ* and flagellated *Monads*; *Pediastreæ* and Algoid filaments. All these are therefore proved, with the greatest certainty, to be interchangeable forms which may be assumed on different occasions by newly-evolved specks of living matter.

But if all this may take place within our superheated experimental flasks, what wider possibilities are opened up concerning the evolutional powers of the unimpaired organic solutions which exist in all damp places upon the surface of our earth, and in our ponds, lakes, rivers, and ocean beds! Here imagination alone can aid us, and yet analogy stands with her ever-ready though often deceitful torch.

We cannot ignore the fact that such solutions as we have employed in our experiments are not productive of any much greater variety of organisms even when exposed to the air; whilst unheated infusions of vegetable matter (such as portions of aquatic plants or young twigs of land plants) readily teem with all that endless variety of organic forms which proves so

enchanting to those who examine with the microscope the motley inhabitants of ponds and stagnant waters.

Again, the transitions and metamorphoses which have been observed and carefully recorded by innumerable workers as occurring amongst many of these forms, are similar in kind though much easier to substantiate than those which have now been definitely proved to exist between *Bacteria*, *Torulæ*, *Fungi*, *Amœbæ*, *Monads*, and various kinds of *Algæ*.

The admirably complete investigations of Dr. Braxton Hicks[1], Itzigshon and others, had already taught us how close is the alliance which exists between such modes of growth as *Protococcus*[2] and the various fila-

[1] See *Appendix* D, pp. liii et seq.

[2] After speaking of some experiments in which *Protococcus* or the so-called 'green matter' of Priestley made its appearance, Burdach adds (loc. cit., t. i. p. 25):—'Du marbre ayant été également renfermé dans un flacon, avec de l'eau distillée et de l'air atmosphérique, de l'oxygène ou de l'hydrogène, puis exposé à la lumière du soleil à la chaleur du bain-marie, il ne se produisit pas de matière verte, mais une substance mucilagineuse avec de filaments blancs, dont quelques uns étaient ramifiés. Des morceaux de granite qui venait d'être détachés du milieu d'un bloc, et que j'enfermai avec de l'eau distillée et du gaz oxygène ou hydrogène, donnèrent au soleil de la *matière verte*, avec des filaments confervoïdes, et au bain-marie des flocons seulement.' Retzius also observed, as Müller says, that a 'peculiar kind of Conferva was generated in a solution of muriate of baryta in distilled water, which had been kept half a year in a bottle closed with a glass stopper.' *Protococcus* has also been observed by many to form upon the sides of glass vessels containing distilled water when they remain undisturbed for some time in warm weather, and are exposed to sunlight. Prof. Schaffhausen, indeed, even says ('Cosmos,' 1860) that he has seen green *Protococci* develop within hermetically-sealed vessels containing tolerably pure boiled water when the flasks have been exposed to sunlight. These various facts seem

mentary and ulva-like Algæ which were, and are still regarded by various naturalists as so many distinct and constant species. On the other hand, they have also taught us that these supposed autonomous forms are derivable either from Lichens or other Algæ, that they are capable of vegetating for months in one or other of these algoid states, or in several of them successively, whilst at last under suitable conditions such modes of growth may lapse again into those characteristic of Lichens or Mosses. Green corpuscles (gonidia) thrown off from a single Lichen have been seen by Dr. Hicks to assume the forms and mode of growth characteristic of no less than twenty-three supposed species of Algæ. On the other hand, gonidia thrown off from an Alga or from a Moss are capable of going through any similar number of modes of growth, according as the conditions to which they are subjected undergo variations. Speaking generally, heat and drought were observed to be favourable to their development into Lichens, though in damp places some grow and multiply prodigiously in the form of Algæ, whilst others seem to develop into different forms of Moss. In water the gonidia may either continue to grow after the fashion of

to indicate that green protoplasm manifests itself almost as readily under certain circumstances, as colourless protoplasm does under others: and the same may be said concerning the red protoplasmic masses (*Palmellæ*) which in the form of so-called 'blood spots' have at various times been known to make their appearance upon all kinds of provisions. (See Lindley's 'Veget. Kingdom,' p. 446; and Hecker's 'Epidemics of Middle Ages,' pp. 105–207.)

various Algæ, or, as Cohn ascertained by his observations upon *Protococcus pluvialis*, they may from time to time give birth to many more actively moving, animalized forms belonging to the group *Monadinia*. So that as Cohn remarks, after summarizing the results of his observations upon the actual developmental forms assumed by *Protococcus*[1]:—'A critical and comparative consideration of the foregoing facts would therefore appear to render untenable almost all the principles which modern systematists have hitherto adopted as the basis for construction of their Natural Kingdoms, Families, Genera, and Species.'

Views of the kind hitherto announced, startling as they may appear, had previously found favour in the eyes of many philosophic naturalists. And however much such doctrines may have been confirmed and placed upon a more secure basis by recent researches and observations, we may admire the breadth of view and scientific prescience which revealed themselves when the following views were expressed by my respected friend and colleague, Professor Grant, in 'Lectures on Comparative Anatomy' published in 1833[2]. In many, though of course not in all respects, they closely represent our present state of knowledge on the subjects to which they refer. He said:—

[1] For an enumeration of these surprising metamorphoses, see *Appendix* D, p. lxxxii.

[2] In 'Lancet,' vol. ii. p. 1001.

'All forms of matter appear to have a tendency and a capability to become organized, as all organic forms tend to higher stages of development, and chemical analysis shows the highest as well as the lowest forms of organic beings to consist of a complicated aggregate of mineral gases and liquids and solids. These organized aggregates once formed from their elements, all possess alike the means of transmitting their forms by generation, which is effected by the separation of a portion of their substance, when their own development is completed. * * *

'Although no animal can exactly produce its like, the progeny are so nearly such that, for all the purposes of science, we regard their forms as identical with those of the parent, and out of an indefinite series of such generations, and of individuals as nearly resembling them, we frame our organic species, and ascribe them to nature. * * *

'The organs of nutrition and relation which we have been hitherto considering, enable the individuals of species for a limited time to live, to grow, and to feel; but while myriads of individuals appear and disappear, like passing shadows, in rapid succession, the species, or the typical forms of groups of animals, are still prolonged on the earth. The species, however, like the individuals which compose them, have also their limits of duration.

'The life of animals exhibits a constant series of changes, which occupy so short a period, that we can

generally trace their entire order of succession, and perceive the whole chain of their metamorphoses. But the metamorphoses of species proceed so slowly with regard to us, that we can neither perceive their origin, their maturity, nor their decay, and we ascribe to them a kind of perpetuity on the earth.'

PART III.

HETEROGENESIS.

CHAPTER XVI.

ANCIENT AND MODERN VIEWS CONCERNING HETEROGENESIS.

Organic Morphology. Meaning of Heterogenesis. Views of Aristotle and others. Modified in more Recent Times. Doctrines of Needham and Buffon. Inconsistencies of the latter. Views of O. F. Müller, Treviranus, and Tiedemann. General Doctrines of M. Pouchet.

Division of the Subject. Synthetic and Analytic Heterogenesis. Similar differences amongst Fermentations. Origin of 'vital' Forces. Their Mode of Expenditure. Analytic and Synthetic changes during Growth. Influence of pre-existing Protoplasm. Dependence of Life upon Decomposition. Views of Liebig, Freke, and Hinton. Many 'vital' Processes allied to Fermentations. Natural Tendencies to the Formation of 'living' Matter. Peculiarities of Vital Processes. Distinct though Related Activities in Molecules, Cells, and Organs. Characteristics of Health and Disease. Conditions favourable to the Occurrence of Analytic Heterogenesis.

THE problems which now demand our attention are somewhat different in nature, though they are not less replete with interest than those which have been hitherto considered.

It has been proved in the only way in which such a fact could be established, that 'living' matter is formable from its elements, and that the highly com-

plex molecules of which it is composed are the results of chemical combinations—brought about by the same physical agencies that suffice to engender other less complex compounds from similar elements. And now we have to follow up our studies concerning the nature of the forms which this new-born living matter tends to assume, and the modifications which the forms are capable of undergoing—we have, in fact, to build up that portion of the subject which, if it applied to crystals instead of organisms, would come under the head of Crystallography. Having reference to living things, the inquiry constitutes the empirical basis of Organic Morphology.

As we have already pointed out[1], under the old beliefs in 'spontaneous generation,' there either were or ought to have been included, two entirely distinct processes. First, although less talked of than the other, there was the process which we have called Archebiosis, whereby living matter originates 'spontaneously;' and secondly, there were the processes of Heterogenesis, whereby the matter of already existing living things gives birth to other living units wholly different from themselves, and having no tendency to assume or revert to the parental type. It is this latter aspect, therefore, of the old doctrines concerning 'spontaneous generation' which we have now to consider.

The belief in the fundamental unity of Life, under every variety of organic form, is one of most ancient

[1] Vol. i. pp. 244, 245.

origin. The nature of the living matter, whether animal or vegetal, was regarded, even by many ancient philosophers, as an accident dependent upon the influence of particular sets of conditions. As we have seen[1], Aristotle thought that plants might be engendered by the tissues of animals, and, on the other hand, that certain lower kinds of animals might take their origin from and within the substance of plants. Ovid, therefore, was but reproducing an actual belief of his time when, in his exposition of the Pythagorean philosophy, he wrote:—

> 'Si qua fides rebus tamen est adhibenda probatis;
> Nonne vides quæcunque morâ fluidove calore
> Corpora tabuerint, in parva animalia verti?
> I quoque, delectos mactatos obrue tauros;
> Cognita res usu; de putri viscere passim
> Florilegæ nascuntur apes.'

And, under the particular form alluded to by the poet, the doctrine has been handed down by some even to our own times. The higher organisms, both animal and vegetal, after their death, and during the process of putrefaction, have been supposed to be capable of giving rise indifferently—and often at the same time—to certain of the lowest animals and of the lowest plants.

Before inquiring into the nature of the more recent and exact information obtained upon this subject, it will, we think, be well to cite the opinions of a few

[1] See vol. i. p. 253.

of the older naturalists and physiologists who have written during the last hundred years. We shall thus see how strong the belief in the truth of this doctrine has been, amongst many of those whose opinions carried great weight in their time.

Commencing with Needham, the English champion of heterogeny during the last century, we find him maintaining a belief in the essential oneness of the living force or vital principle of both animals and plants—'*force végétative*' as he called it. This, he thought, always survived after the death of the particular animals and plants in which it had previously been the guiding principle. Restricted in its operations during the life of the individual—that is, acting in a determinate way in each given organism—it assumes more freedom of action after the death of the organism. Still residing, however, in the organic matter, it forces the complex molecules of such materials to enter into new 'living' combinations—the actual nature of these, and, consequently, of the resulting living things, being dependent upon the conditions in which the organic matter is placed and the particular sets of physical influences to which it is subjected [1].

It will be seen that this is an essentially spiritualistic conception. We shall find, however, that Buffon, who was for a time associated with Needham, and who was

[1] See Spallanzani's 'Opuscules,' Exposition des nouvelles idées de M. de Needham sur la système de la génération, t. 1er, chap. 1er.

much influenced by his views, preferred giving them a much more materialistic acceptation. Buffon's '*molécules organiques*' may be said to replace the 'force végétative' of Needham. His views on this subject are so interesting that we shall quote them somewhat fully. 'My researches and experiments upon organic molecules,' he says[1], 'demonstrate that there are no pre-existing germs, and at the same time they prove that the generation of animals and of plants does not take place after any single fashion. There are, perhaps, as many beings, whether animal or vegetable, that are produced by a fortuitous concourse of organic molecules, as there are animals or vegetables which can reproduce themselves by a constant succession of generations.

'The organic molecules—always active, always persistent—belong as much to plants as to animals. They penetrate brute (dead) matter; they excite changes within it; they influence it in all its parts; they make it serve as the basis for an organized tissue, of which these living molecules are the only active principles. They are only under the subjection of a single power, which, though passive, directs their movement and fixes their position. This power is the mould, or intimate pattern, of the particular organized body. The living molecules which the animal or the plant draws from its nutritive materials, or from its sap, incorporate themselves with all parts of the material mould; they carry with them the powers of growth and life; they

[1] Supplement, 'Histoire de l'Homme,' 1778, t. viii.

make this mould live and grow in all its parts. The internal and intimate form of the mould, in all organized beings, alone determines their movement and their relative position during the phenomena of nutrition and development.

'And, when death extinguishes the fire of organization—that is to say, the power or influence of the mould—decomposition follows; whilst the organic molecules, which all survive, finding themselves at liberty during the dissolution and putrefaction of the body, pass into other bodies as soon as they are brought under the influence of some other organic mould. Thus they are able to pass from animal to vegetable, and from vegetable to animal, without alteration, and with the constant and ever-active power of bearing with them nutritive phenomena and life. Only, there occurs an infinity of *générations spontanées* in the interval during which the power of the organic mould is in abeyance—that is to say, in that interval of time during which the organic molecules find themselves at liberty in the midst of the tissues of dead and decomposing organisms, and whilst they have not been assimilated by the moulding power of organisms belonging to ordinary species of animals and plants. Such organic molecules, always active, strive to affect the putrefying matter; they appropriate some inert particles, and produce by their union a multitude of small organisms, some of which, such as earth-worms, fungi, etc., appear as tolerably large animals and vegetables, whilst others, almost

infinite in number, are only visible by the microscope. All these bodies only come into existence by a spontaneous generation, and they fill the gap that nature has left between the simple living organic molecule and the animal or the vegetable. Also, there are to be found all degrees, all imaginable shades, in this series—this chain, which descends from the most highly-organized animal to the simple organic molecule. Taken by itself, this molecule is far enough removed from the nature of an animal; taken in combination, these organic molecules would be removed quite as much if they did not appropriate inert particles, and if they did not dispose[1] these after a certain fashion in accordance with the intimate pattern of some animal or of some plant. And as this form-arrangement ought to vary infinitely, in consideration alike of the varying number and of the different action of the organic molecules upon the inert matter, there ought to result, and there do in fact result, beings of all degrees of animality. And this spontaneous generation (to which all these beings alike owe their existence) comes into play, and reveals itself whenever organized creatures undergo decomposition. It comes into play universally after their death, and sometimes also during their life when there are certain defects in the organization of the body, such as hinder the inner mould [or plastic force] from absorbing and

[1] Previously, the very reverse of this was said. The mould or pattern was the passive (?) power, in obedience to which the incorporeal 'molécules organiques' arranged themselves.

assimilating all the organic molecules contained in their food. These superabundant organic molecules, which are unable to penetrate into, and thus nourish, the animal organism, strive to unite themselves with certain particles of inert alimentary matter, and thus, as during the process of putrefaction, form certain organized bodies. Such is the mode of origin of Tapeworms, of Ascarides, of Flukes.' . . .

Buffon, not always logical and consistent, was notoriously a bold and untrammelled thinker, though he held a very inferior place as an actual observer. Generalization was more to his taste than the laborious and less inviting occupation of acquiring the necessary data; and he did not always restrict himself to theories which reposed on a solid basis of fact. This doctrine of his, which we have just quoted, is a strange mixture of Platonic, Leibnitzian, and materialistic philosophy. His 'moule intérieur' is represented as an actual power, corresponding in some respects with the Platonic 'Idea;' whilst his 'molécules organiques' are in other respects similar to the 'Monads' of Leibnitz—though, like the *νοῦς* of Anaxagoras, they are represented as movers of matter, rather than as essential and sole constituents of a self-moving matter. The notion of Needham, however, was much more assimilable with our own doctrines. Believing in the influence of 'external conditions' on putrefying organic matter, as Needham did, his postulation of a single active 'force végétative' was a superfluity—a remnant

of the old vitalistic theories which may now be lopped off without further concern—leaving us no other life-factors in the case of these lowest organisms than the organic matter and the external conditions or incident physical forces.

But let us glance more briefly at the doctrines of two or three of those who succeeded Needham and Buffon.

O. F. Müller, one of the most distinguished naturalists of his time, was also a believer in the spontaneous generation of the lower kinds of organisms. On the present aspect of our subject he expresses himself most distinctly thus[1]:—'Animals and vegetables decompose into organic particles, endowed with a certain degree of vitality, and constituting the simplest animalcules, which are capable of developing, either after the fashion of germs, by union with other particles, or by themselves contributing towards the development of some other animal—only to become free again after its death, and to recommence eternally a similar cycle of mutations.' At the commencement of the present century, also, Treviranus[2] expressed his belief in the existence of a primitive amorphous organic matter—a plastic material which was ready to assume all the forms met with in living things, and which was most prone to alter its present pattern under the powerfully modifying influence of a change in the conditions of its existence.

[1] 'Animalcula infusoria, fluviatilia et marina,' &c. Opus posth. Leipzig, 1787.

[2] 'Biologie,' Göttingen, 1802, tom. ii. pp. 267, 403.

The celebrated Tiedemann also gave most definite expression to his views on this subject when he said:—'Organized beings are produced from others like themselves, or else they owe their origin to organic substances in a state of decomposition.' Whilst, farther on[1], he adds:—'The plastic power of the matter is not extinguished after death; it preserves the faculty of clothing itself again in a new form, and of displaying its aptitude to manifest life. Death falls then only upon the individual organizations, whilst the organic substances entering into the composition of these beings, continue able to assume form and to receive life.' Respecting the conditions leading to, or preventing, this new assumption of living forms and properties, Tiedemann says[2]:—'The organic materials which become separated from an organism preserve—when they are not reduced to their elements, or converted into binary compounds by the action of chemical affinities—the property of reappearing, through the concurrence of favourable external conditions (of heat, of water, of air, and of light) under more simple animal and vegetable forms varying always by reason of the influences to whose action they are submitted.'

Bremser and Burdach—other German physiologists—were also firm believers in the doctrines of Heterogenesis, but as we have already referred to them[3], we will now pass to the consideration of M. Pouchet's

[1] 'Physiologie de l'Homme,' Paris, 1831, tom. i. p. 100.
[2] Loc. cit., p. 104. [3] Vol. i. pp. 246 and 261.

general doctrine concerning heterogeny. He says[1]:—'It may be considered as a fundamental law that phenomena of fermentation, or of catalytic decomposition, precede or accompany every spontaneous generation . . . Organisms are only produced from expiring nature itself, and at the moment when the elements of the beings upon which they are engendered enter into new chemical combinations, and undergo all the phenomena of fermentation and putrefaction[2] . . . . It thence results that primary generations are only manifested after the bodies from which they are derived begin to undergo the initial stages of decomposition; as if the new beings, to become organized, awaited the disintegration of others, in order that they might avail themselves of the molecules of the dying organism as soon as these were set at liberty.' Thus, then, under the sway of fermentation or of putrefaction, 'the organic molecules of organized beings are decomposed and separated; and, after having wandered at liberty during an unlimited time, whenever *les circonstances plastiques* begin to manifest themselves, these molecules group themselves afresh in order to constitute a new being[3].'

[1] 'Hétérogénie,' Paris, 1859, p. 335.

[2] Ibid. p. 136.

[3] But M. Pouchet did not range himself with Lamarck and others, who believed that physical forces alone were capable of bringing about Life and organization in dead inorganic matter; on the contrary, he professed his belief in the activity of 'une force plastique,' or special 'force vitale.' He says:—'Si dans nos expériences, c'est au contact de corps divers que se developpent les Proto-organismes, il ne faut pas

These quotations will suffice to show the amount of favour with which doctrines of heterogeny were regarded by many of our predecessors, and also to indicate the nature of some of the problems which remain to be investigated.

It will be found, however, that the facts which we have to consider may be ranged under two distinct categories. We have to study processes that may be classed under the head of Synthetic Heterogenesis, and others belonging to what may be called Analytic Heterogenesis. The latter set of changes may, for the sake of convenience, be studied under two heads:—

1. *Synthetic Heterogenesis* refers to the origin of larger and somewhat more complex forms of life, by a process of fusion, with molecular re-arrangement, taking place amongst the simplest living units.
2. *Analytic Heterogenesis* refers to the origin and presence of some of the simplest forms of life from and within the bodies of other organisms:
   a. Within the bodies of higher animals and plants.
   b. Within the substance of lower organisms, both animal, protistic, and vegetal.

croire que la raison de leur apparition est absolument sous l'influence des affinités; ce serait rabaisser la création au niveau d'une attraction chimique. Non, la cause intime de la vie, cette force initiale qui en groupe le canevas est cet esprit que Bremser considère comme le régulateur de tous les actes biologiques.' In fact, M. Pouchet definitely professes to be a 'vitalist,' and says (p. 428), 'I have always thought that organized beings were animated by forces which are in no way reducible to physical and chemical forces.'

This division may remind the reader of our classification of fermentative processes [1]. The analogy existing between them is very close; so close, indeed, that we should have been quite entitled to have set down a third class of processes under the name of Analytico-Synthetic Heterogenesis. We wish, however, to simplify our statements as much as possible, so that for the present we make no further comments on this latter possible division of the subject. The analogy which exists between Synthetic Fermentation and Synthetic Heterogenesis will be much more fully understood after the perusal of the next chapter; meanwhile it may be useful for us to make a few remarks with the view of throwing light upon the more popular aspect of the question—the processes of Analytic Heterogenesis.

We have previously endeavoured to show that all the processes or functions carried on in animal bodies are effected at the cost of organic materials which are assimilated in the form of Food [2]. These complex products are decomposed, so that 'forces' are liberated as molecular movements; this liberated molecular motion communicates itself to the elements of the tissues and organs, and supplies the motive power by which those functions are carried on which go to constitute vital activity. It may reveal itself, for instance, in a display of muscular or nervous power, whereby the organism responds to impressions made upon it from without; or

[1] See vol. i. p. 423. [2] Vol. i. pp. 23–49.

it may be consumed in the work of secretion. But it must also be remembered that portions of such liberated motion may be consumed in carrying on the work of nutrition and growth—that is, in carrying on those acts by which the matter of the organism is either renovated or increased in quantity, with or without an increase in the complexity of its structure. Similar molecular energies are also to a less extent set free by changes in the active or functioning matter itself—since all action implies more or less of alteration in the molecular structure of the substance which manifests it.

Thus, whilst one portion of the assimilated food is being decomposed, another is being simultaneously elevated in the scale of complexity, and is fashioned into the likeness of the matter to whose influence it is exposed. Molecular movements of a special kind are constantly taking place in each growing tissue, and the molecules of adjacent organic matter contained in the nutritive fluids with which they are brought into contact, seem (whilst obeying an inherent tendency to enter into 'living' combinations) to be coerced to fall into living matter similar to that of the tissue itself[1]. That is to say, whilst living matter is formed as a result of an inherent tendency of the food molecules to enter into such modes of combination, the peculiar kind of living matter which is produced is attributable to the influence of that with which it comes into

[1] See remarks concerning the assimilating processes carried on in an *Amœba*, p. 132.

contact—whether we have to do with independent organisms, or with mere subordinate units entering into the composition of the muscles, nerves, or glands of some higher organism.

Some of these views have been previously advocated by others. Thus, Liebig has pointed out [1] that 'the animal metamorphosis is itself a main cause of the alterations which the food undergoes, and a determining condition of the nutritive process.' And the dependence of the phenomena of Life upon decomposition has also been ably argued by Dr. Freke and Mr. Hinton. The decomposing matter is supposed by them to render active an amount of force which helps to give rise to new living compounds [2]. Our position, that during the growth of organisms living matter is formed, partly in obedience to natural tendencies possessed by certain kinds of molecules to enter into such modes of combination, and partly under the immediately fashioning influence of the pre-existing living matter, will be better understood after some additional words of explanation.

Liebig has frequently called attention to the fact

[1] See vol. i. p. 426.

[2] Mr. Hinton says ('Life in Nature,' p. 238):—'As one example, let us take the germination of the seed. Put into conditions which elicit or permit the operation of the chemical affinities, it begins to decompose. The downward or approximative motion thus arising, imparted to other elements in the seed which are so constructed as to admit of motion most readily in the opposite or vital direction, becomes in these elements a motion of life or growth.'

that many of the processes which habitually take place in living organisms are essentially similar to processes of fermentation. It is, indeed, commonly recognized that the salivary and various other alimentary secretions when mixed with the elements of the food during digestion, incite processes essentially fermentative in nature. The transformation of starch into sugar, which takes place during the germination of the seeds of cereals and other plants, may be considered to belong to the same category. Further, Liebig says[1]:—'Many plants with woody stems are found to contain, in autumn, a matter perfectly like the starch of potatoes, or of the cereals, deposited in the substance of the wood, which in the spring, when the plants re-awaken to life, becomes converted into sugar. The ascending juice of the maple is so rich in sugar, that in regions where this tree occurs in such numbers as to form forests, its juice is employed in the manufacture of sugar.' And, again, he adds:—'The maturation, as it is called, or sweetening of winter fruits, when stored up for their preservation in straw, is the result of a true fermentation. Unripe apples and pears contain a considerable amount of starch, which becomes converted into sugar by the nitrogenous constituent of the juice passing into a state of decomposition, and transmitting its own mutations to the particles of starch in contact with it.'

There are, therefore, undoubtedly many striking

[1] 'Letters on Chemistry,' 1851, p. 201.

resemblances between the changes which are carried on in living bodies and those which are ordinarily classed under the head of fermentations. We have shown that, in the commoner kinds of fermentations, processes of analysis and processes of synthesis go on simultaneously in the fluid; and, similarly, we have found that growth of pre-existing protoplasm is intimately associated with simultaneous processes of decomposition or analysis [1]. We have proved that some of the products of this process of synthesis which takes place in ordinary fermentations appear in the form of living matter (even where no such matter pre-existed), so that we are compelled to believe that there is a natural tendency to the formation of the compounds which constitute such matter, just as there is a natural tendency to the formation of simpler chemical combinations. We are, therefore, entitled to believe that

[1] There is, therefore, much reason for the belief that the appearance of living matter, whether it arises independently or by a process of growth, is due in part to the existence of molecular movements which are initiated by chemical decompositions. This state of movement, as Liebig says, is capable of 'being communicated to other atoms in contact with the former, so as to cause the atoms and elements of these latter also, in consequence of the resulting disturbance of the equilibrium of their chemical attraction, to change their position, and to arrange themselves into one or more new groups.' Or, as Mr. Hinton puts it:—'One body is ceasing to be organic, and therein is giving off its force, and in immediate connection with it another body is becoming organic, and therefore is receiving force into itself. Can we be misinterpreting these facts in saying that the former process is the cause of the latter; and that the decay gives out the force which produces the growth.' ('Life in Nature,' 1861, p. 43.)

where new living matter is produced during the growth of organisms, this is in part brought about because there is a natural tendency amongst the food molecules to fall into such states of combination—however much the particular nature and form of the new matter may be determined by the influence of that which already exists.

It must be remembered, however, that although many of the processes whereby food is assimilated within the bodies of living things are strictly comparable in nature to processes of fermentation, nevertheless, the precise changes which the elements of the food undergo in the various stages of the process are quite different from those which take place in ordinary fermentations. And, moreover, in the last stage of the process, when food has been reduced to the condition of blood-plasma, the latter is subjected throughout the body to all the special activities of the several tissues, and is formed into as many kinds of living matter, which subsequently, under the influence of 'organic polarities,' fashion themselves into the exact likeness of these several tissues[1].

[1] In the 'Introduction' of his celebrated 'Règne Animal' (1816), Cuvier made the following notably suggestive remarks, which will be found to be very much in accordance with our present argument. He says:—'Life, then, is a vortex (*tourbillon*), more or less rapid, more or less complicated, the direction of which is constant, and which always carries along molecules of the same kind, but into which individual molecules are continually entering, and from which they are constantly departing; so that the form of a living body is more essential to it than its matter. . . . As long as this movement subsists, the body in which

Such are the diverse and marvellously complex processes from moment to moment taking place within us, and which, by their combined effects, contribute to make us such creatures as we are. All organisms are more or less complex wholes made up of multitudinous and independent units, differing in nature and variously combined, though all working harmoniously, and tending to produce the characteristics of the organism of which they form part. But these several independent units are again made up of an aggregation of living molecules; and the molecules of the cell bear, in fact, to its activity, just the same relationship that the cells bear to the organ which they help to compose, and that the organs bear to the organism of which they form part. This whirl within whirl of activities of all kinds, repeatedly subordinated in the most complex manner, goes to make up the 'Life' of all higher animal organisms; so that, although the phrase may not be sufficiently exclusive to constitute a definition of Life, this state is very aptly epitomized by speaking of it as a 'coordination of actions[1].' So long as all these activities manifest themselves in an appropriate manner, so long as there is a perfect coordination, the

this takes place is *living—it lives.* When it is permanently arrested, the body *dies.* After death, the elements which compose it, abandoned to the ordinary chemical affinities, are not slow to separate; from which, more or less quickly, results the dissolution of the body that had been living. *It was, then, by the vital motion that its dissolution was arrested, and that the elements of the body were temporarily combined.*'

[1] Herbert Spencer's 'Principles of Biology,' vol. i. p. 60.

organism is said to be in a healthy state, and its 'vital powers' are good. Under these circumstances, it grows in the most regular manner, with a constant tendency to follow in the grooves along which its predecessors may have gone. But when from any cause the central controlling or coordinating apparatus is anywhere weakened, morbid or unnatural processes may begin to take place within and amongst the tissue elements of the liberated region. These previously subordinate units cast off their subordination, and inflammatory or degenerative processes may occur. On the other hand, where the health of the whole organism is lowered, where the 'vital powers' are on the wane, and all coordinating activities are checked, the special activities of the separate units are similarly diminished, and their elements are more free to enter into such less specialized combinations and modes of activity as living matter is prone to assume when it arises independently in the midst of a fermenting infusion. That is to say, as soon as the multitudinous maelstrom-like activities—whirl within whirl—which are usually so potent in the tissues of a healthy organism become decidedly diminished in intensity, the newest and least specialized portions of living matter entering into the composition of these tissues, become more and more capable of assuming those modes of growth which new-born living matter tends to assume when uninfluenced by the processes taking place in the matter of any pre-existing living unit with which it may be in contact.

These views are thoroughly harmonious with the facts; since, as previous quotations may have indicated and subsequent details will show, the phenomena of Analytic Heterogenesis are manifested in an increasing degree when the higher organisms in which they appear are sickly, dying, or actually dead.

CHAPTER XVII.

SYNTHETIC HETEROGENESIS.

The 'Pellicle.' Biocrasis. Observations of Pineau and Pouchet. Author's Observations. Formation of Embryonal Areas. Origin of Amœbæ and of Fungus-spores. Mode of Origin of Embryonal Areas. Direct origin of Monads and Fungi from same Elements. Development of Flagellum. Indirect Origin of Monads. Their Transformation into Amœbæ. Encystment. Death of others by Analytic Heterogenesis. Origin of other Amœbæ by Archebiosis. Subsequently break up into Fungus-spores. Similar Fungus-spores arising by Archebiosis. Their Germination. Mutual interchangeability between Monads, Amœbæ, and Fungus-spores. Further proof. Fungus-spores, or Monads and Amœbæ, producible at will from Embryonal Areas. Embryos of Unknown Organism. Origin of Enchelys seen by Pineau. Origin of Paramecium described by Pouchet. Confirmation by Joly and Musset. Essential Conditions. Author's Observations on Origin of Paramecia. Fission of Embryos. Their Minute Structure. Great Simplicity. Subsequent Modifications. Origin of Vorticellæ. Pineau's Observations.

Theoretical objections. These fostered by Novelty of the Facts. Similar Difficulties concerning Formation of Crystals and Development of Higher Organisms. Diversity of 'Spontaneous' Products. This natural and long recognized. Infusorial 'Species' Convertible. Mutability an Essential Characteristic. Facts recorded easily Verifiable. Synthetic Heterogenesis comparable with the Synthesis occurring in Archebiosis.

THE mode of formation of the 'primordial mucus' of Burdach—the 'proligerous pellicle' of Pouchet—has been already described; but it now remains for us

to give some account of the various changes that are apt to occur in this aggregation of living units which so soon collects, in the form of a scum, upon the surface of nearly all infusions of organic matter.

The pellicle is composed for the most part of a dense aggregation of *Bacteria* of various sizes and shapes imbedded in a more or less abundant, pellucid, gelatinous material. Very frequently there are also a variable number of intermixed *Vibriones* and more or less characteristic *Torulæ*[1]. The *Bacteria* in this layer are mostly placed vertically to the surface, so that an examination of the upper surface under the microscope generally presents the appearance of a stratum densely studded with small, though tolerably uniform granules. On attempting to remove a portion of this pellicle, it is found to constitute a more or less coherent membrane.

It is now a well-known fact that when two or more Amœbæ come into close contact with one another, they may fuse so as to constitute a larger individual of the same kind, which afterwards creeps about and seizes food as its component parts had previously done. Such a process must be classed under the head of Homogenetic Biocrasis[2]; for, although separate living units fuse to form a new individual, the process is one of mere fusion, and the product

[1] The different kinds of 'pellicles' are more fully described by M. Pouchet in his 'Hétérogénie,' pp. 355-367.

[2] See vol. i. p. 233.

is similar in kind although necessarily larger than its components.

Similar mutual attractions, however, may be exerted by other living units when brought into close contact with one another, and the result may be the formation of an aggregate in which considerable molecular changes are compelled to take place. The products resulting from such a fusion may be quite different from the originally fused units; whilst they will differ at different times according to the precise nature and number of the units which enter into combination. Such processes are frequently to be observed taking place in various parts of the 'proligerous pellicle.' It is in this way, in fact, that those phenomena occur which make the name 'proligerous pellicle' suitable for the scum that forms on organic infusions. The processes themselves come under the head of Heterogenetic Biocrasis.

The first person who actually described the microscopical appearances characterizing the evolution of higher organisms from the pellicle, was M. Pineau. This he did in 1845, in a memoir entitled, 'Récherches sur le Développement des Animalcules Infusoires et des Moisissures [1].' More precision, however, was given to the subject in 1859, by M. Pouchet, when he described in his 'Hétérogénie,' the mode of origin of some of the organisms which had formed the objects of Pineau's investigations, as well as of some different organisms.

[1] See 'Ann. des Sc. Nat.' (Zoologie), t. iii. p. 182, and t. iv. p. 103.

Although some of these observations have not been recorded with all the details which might have been desired, yet I have satisfied myself that the statements made by Pineau and Pouchet are substantially correct.

Pineau has given an account of the mode of origin of the microscopic fungus known as *Penicillium glaucum*, and also of *Monas lens*, in addition to other organisms to which we shall subsequently refer.

M. Pineau watched the various stages in the evolution of *Penicillium glaucum* in the midst of a granular pellicle which formed on an infusion of bread, after it had

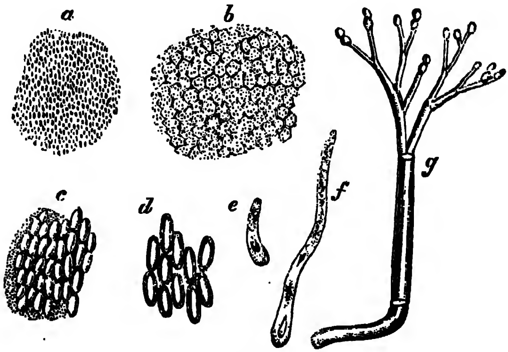


Fig. 51.

Mode of Origin of *Penicillium*. (Pineau.) (× 400.)

a. Granular Pellicle.
b. First appearance of Germs.
c–g. Illustrating their subsequent development into *Penicillium*.

undergone the acid fermentation. Indistinct networks of various sizes, with roundish or slightly polygonal

meshes $\frac{1}{8300}''$ in diameter, at first made their appearance, and after a lapse of twelve hours the individual units became more distinct and began to assume an oval form. Whilst still aggregated together, they were noticed slightly to increase in size. After a time they separated from one another, and then began to elongate into filaments which gradually displayed the characters of *Penicillium glaucum*. Pineau pointed out that the resemblance between these inferior members of the animal and of the vegetable kingdoms is so close, 'qu'il est impossible de distinguer une monade d'un globule mycodermique dans les premières phases de leur developpement.'

The specimens of *Monas lens* made their appearance in a pellicle which formed on an infusion of veal. They appeared first in the midst of it 'as an indistinct areolar network, the meshes of which were about $\frac{1}{8300}''$ in diameter.' This network gradually became more distinct, owing to the contours of its component cells becoming more clearly defined. These at last separated from one another, and then each revealed a fine whip-like filament proceeding from a part of its circumference. The individual corpuscles, which were at first quite stationary and in contact with one another, exhibited slow oscillating movements as they separated [1].

[1] He says, one sees first 'de petits amas de granulations dont les contours commençaient par être diffus; peu à peu ces amas devenaient plus nettement circonscrit et ils finissaient par acquérir l'aspect de véritables Monades, d'abord immobile, puis douées de mouvement.'

The corpuscles gradually moved more briskly; and finally, detaching themselves altogether from the pellicle, they became free-swimming animalcules each of which had a rapidly vibrating flagellum. Their development frequently took place in groups of various sizes, as above described; but when the solution contained less organic matter, or when the weather was colder, single isolated corpuscles were frequently seen developing in different parts of the pellicle.

The description given by M. Pouchet of the mode

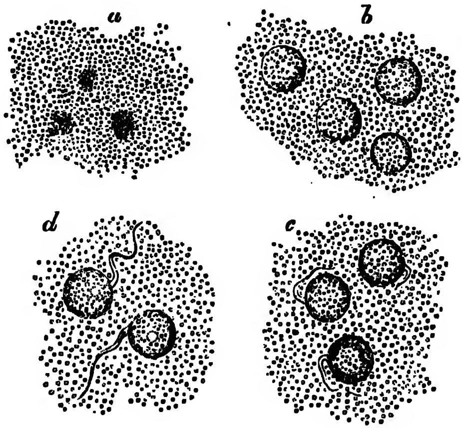


FIG. 52.

Origin of *Monas lens.* (Pouchet.) (x 1800.)

a. First Stage.
b. Tailless Corpuscles.
c. Corpuscles with motionless tails.
d. Fully-developed Monads.

of evolution of *Monas lens* is essentially similar. He observed the first rudiments of them in the pellicle of

an infusion of hay on the fourth day, and they were then about the same size or even smaller than those mentioned by Pineau. On the sixth day they were somewhat larger ($\frac{1}{2500}''$)—each displaying a tail and well-marked tremulous movements, whilst it exhibited in its interior, besides the usual small granules, a transparent vesicle about $\frac{1}{9000}''$ in diameter. On the seventh day they had all detached themselves from one another and from the pellicle, they had increased somewhat in size, and had changed their spherical for a more or less distinctly ovoidal form.

These observations, so far as they go, are similar in many respects to my own; but before dwelling upon them further, I will again describe some of my observations which were published in 1870[1]. These were made, during the previous winter months, upon the 'proligerous pellicles' that formed on two or three hay infusions which had been prepared with hot water[2].

In a pellicle which previously presented a uniform appearance, certain areas, altogether irregular in size and shape, but always presenting outlines bounded by curved lines, gradually made their appearance. These were at first distinguishable from the general groundwork of the pellicle only by their somewhat lighter aspect. On careful microscopical examination with high powers, it was seen that the boundary of such an area—measuring perhaps as much, or more than $\frac{1}{300}''$

[1] In 'Nature,' No. 35.

[2] At a temperature of 140°—160°F.

in diameter—was pretty sharply defined from the sur rounding unaltered granular stratum. The immediately contiguous granules of this stratum were occasionally somewhat more tightly packed, though at other times no such change was observable. In either case the unaltered portion of the pellicle was quite different from the included lighter area, because in this an increase had apparently taken place in the amount of jelly-like material between the granules, and, as well,

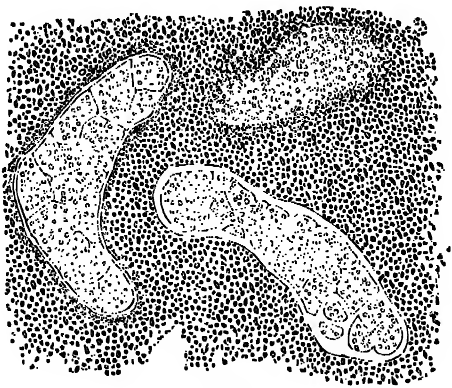


Fig. 53.

Development of Corpuscular Organisms: three areas of differentiation showing different stages. (× 800.)

there was a certain alteration in the refractive index, and occasionally in the size of the granules or altered *Bacteria.* The next change observable was that the included area showed lines crossing it here and there, which at first tended to map it out into certain larger divisions. These intersecting lines gradually increased

in number, till at last the mass became subdivided into an aggregation of rounded or ovoid bodies each about $\frac{1}{5000}''$ in diameter. As these subdivisions were taking place, the mass as a whole also separated from the unaltered pellicle by which it was surrounded. Occasionally there was a distinct interval, at a certain stage, between the parent pellicle and this differentiating mass, whose subdivisions also gradually separated from one another. These subdivisions then appeared as independent corpuscular organisms, bounded by a slightly condensed outer layer, and containing from four to eight of the altered *Bacteria*.

Throughout the winter months such areas of differentiation, and such resulting corpuscular organisms, were frequently met with. The organisms seemed, during such weather, to persist for a very long time without undergoing any notable change (merely, perhaps, increasing somewhat in size); and most of them ultimately became disintegrated without showing any further development [1]. They were always seen in a

[1] Areas formed in the same direct manner, and also without any notable alteration in the refractive index of the contained matter, have recently been seen to appear in the pellicle on a filtered maceration of hay on the second day—this also being during very cold weather. These areas were mostly small, though whilst the process of segmentation was taking place they began to assume a brown colour. This was most marked in some cases, where one end of the area was colourless and the other (the furthest advanced in segmentation) was quite brown. Intermediate portions exhibited the gradual development of the brown colour. The final products of segmentation, after several processes of fission, appeared in the form of small, brown, biloculated fungus-germs, closely resembling those of Fig. 59, *e*.

completely motionless condition, and presented no trace of a cilium—so that they were quite different from the specimens of *Monas lens*. In one infusion of hay in which such organisms had been present for some time, several of them were found to have become spherical and to have undergone a considerable increase in size after a few days of warmer weather. Some were as much as $\frac{1}{2000}''$ in diameter, and the stages in the actual transition of one of these unicellular organisms into an Amœba was seen with the

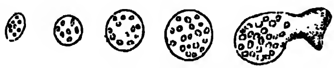


FIG. 54.

Representing gradual enlargement of Corpuscular Organisms, and conversion of one of them into an Amœba. (× 800.)

most perfect distinctness. One half of the organism was obviously amœboid in character, whilst the other half was almost unchanged, containing large granules like those in the unaltered corpuscles. Whilst slow alterations in shape, of a slug-like character, took place in the anterior diaphanous protoplasmic portion, slow rolling movements occurred amongst the granules in the posterior cell-like part, whose matrix seemed to have been rendered more fluid. Having watched this organism for about half an hour, and wishing to examine other portions of the specimen of pellicle in which it had been contained, I moved the glass, and was afterwards unable to find this particular specimen

again. No other Amœbæ or transition states were discovered on this occasion [1].

Subsequently, however, I saw a similar transformation of motionless corpuscles into ordinary Amœbæ taking place in thousands of instances. It occurred in a hay infusion which was examined during one of the summer months. The corpuscles were derived from the pellicle in the same way, and at last separated as colourless ovoid bodies about $\frac{1}{5000}''$ in diameter. They had a slightly condensed exterior layer, but no distinct bounding wall, and seemed to be merely portions of living jelly, in which 5–10 altered *Bacteria* were imbedded. They gradually increased in size, and very shortly a small solid nuclear body began to appear in the interior of each of them. After the corpuscles had attained the size of $\frac{1}{1428}''$ in diameter, their internal substance became more fluid: the previously stationary particles began to oscillate slowly, and they were also smaller and more numerous. Corpuscles which were only a very little larger, began to show slowly-changing irregularities of outline, whilst a vacuole frequently appeared within their substance, lasted about a minute, and then disappeared, to be succeeded by another in a different place. In others the amœboid changes in shape and movements were now quite distinct, whilst the vacuoles were more persistent, and the nucleus had

[1] Prof. Hartig has also described a similar mode of origin of Amœbæ from unicellular organisms, in his observations on the phytozoa of *Marchantiæ*. See 'Journal of the Microscopic Society,' 1855, p. 51.

assumed a ring-like appearance. On the following day they were almost all in active movement as Amœbæ—scarcely any were to be seen in the spherical stationary form. After a few days' exposure to direct sunlight, great numbers of the Amœbæ encysted themselves, though others became filled with minute granules, and seemed to have undergone a process of degeneration[1].

In other cases areas of differentiation, commencing in a manner somewhat similar to what I have already described, were seen to terminate in the production of Fungus-germs. Their mode of evolution from portions of a pellicle found upon a rather old infusion of hay, was also described on a former occasion[2]. The development of a brownish tint in the earlier stages of the transformation made it more easy to detect its real nature. The areas which began to differentiate were generally not very large. They were at first quite colourless, and the granules were separated from one another by a notable amount of transparent jelly-like material. The granules themselves were mostly shaped like the figure 8, and each half was about $\frac{1}{30000}''$ in diameter. A later stage was apparently seen in other areas which had assumed a very faint brownish tint, and presented evidences of a commencing subdivision. As this process of segmentation progressed, the brown tint became gradually deeper. Ovoid masses were frequently seen about $\frac{1}{2000}''$ or $\frac{1}{1000}''$ in diameter, of a decidedly brown

[1] See p. 222.
[2] 'Nature,' 1870, No. 35.

colour, and composed of from eight to twelve or more ovoid subdivisions. In the later stages of the process of multiplication, the individual segments lost all trace of their original granular condition. They became quite homogeneous and highly refractive masses of a

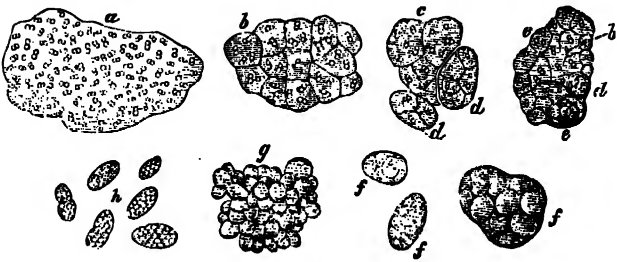


Fig. 55.

Mode of Origin of Germs of Fungi from differentiating portions a Pellicle formed on an Infusion of Hay. (× 800.)

a. First Stage.
b. Second Stage—more refractive, brownish matter undergoing segmentation.
c, d, e, f. Further Stages of segmentation.
g. Ultimate Products of segmentation, which gradually develop into perfect germs (*h*).

brown colour, and looked almost like large brown fat globules. At last, multiplication still proceeding, the mass gradually became resolved, leaving only an irregular heap of spherical or ovoidal bodies of various sizes. The individual segments then increased in size, and gradually became less refractive and lighter in colour. A slight internal mamellonated aspect also made its appearance, as they assumed the form of ovoid

bodies about $\frac{1}{1000}''$ in diameter. Even after attaining this stage of development, some of them occasionally underwent a process of division; and though the majority of them seemed to undergo no change, others, after a time, gave origin to ordinary mycelial threads.

After relating these observations, the following remarks were made:—

'The changes which I have described represent, I think, only two extreme types of a mode of metamorphosis which is apt to take place in portions of the pellicle. In the one case a certain area of the pellicle, after undergoing some changes, resolves itself into a number of ovoid bodies, which collectively are about equal in bulk to the altered area itself; whilst, in the other case, at different stages, the segments of the altered area undergo a process of growth and subdivision, so that, ultimately, the mass of spores which results far exceeds in bulk that of the original area when it began to undergo change.

'At other times intermediate processes are met with; and then fungus-spores are produced after a fashion more closely resembling that which leads to the production of the unicellular organisms above described. The areas of change are then larger than those last described and colourless throughout, whilst the processes of growth and multiplication are less marked at the different stages. Where fungus-spores result after this fashion, the changes in the refractive index, and the homogeneous appearance previously alluded to, still

generally manifest themselves at the ultimate stage of division, though nothing of this kind shows itself in the more simple process leading to the production of the unicellular organisms.'

Subsequent experience has abundantly confirmed the truth of the views then expressed, as I have since seen many changes in the pellicle which were strictly intermediate between the extreme forms just described. The characters of the pellicles that form on different hay infusions of the same strength, differ notably according to the temperature of the water with which the infusions have been made, and, to a less extent, according to the mean atmospheric temperature to which they are subsequently exposed. If the infusion has been prepared with very hot water (140°F and upwards), only a thin and somewhat tough pellicle will form, secondary changes will take place in it very slowly, and they will lead only to the evolution of products of a certain kind. When prepared with moderately hot water (120°F) or with cold water (60°—70°F), the pellicles which are produced become thicker and thicker, and continue for a long time to be soft and pulpy. The changes that may take place in a pellicle of the latter kind are very varied, so that it may give rise to a multiplicity of organic forms.

For a long time my observations were carried on upon infusions made with hot water, and they were also conducted during the winter months, so that the secondary changes which I was able to observe in the

pellicle were neither varied nor numerous[1]. That which is to follow in this chapter concerning my own observations, has been learned from an investigation of the changes in pellicles which form on filtered hay infusions prepared both with warm and with cold water.

In all cases, and at whatsoever temperature the infusion may have been prepared, the earliest change which takes place in the pellicle is such as I have previously described. In certain portions of it—altogether irregular in size, shape, and distribution—the aggregated *Bacteria* begin to form around themselves a certain amount of pellucid, gelatinous matter in which they become imbedded. This change may be well seen in pellicles made with hot water, because such areas continue (more especially when the atmospheric temperature is low) for several days without undergoing much alteration. The *Bacteria* in them are slightly separated from one another, rather larger in

[1] During this time I was also working at the subject of Archebiosis, and I had not then ascertained that even in this part of the investigation infusions are more efficacious if prepared with moderately hot (120°—130° F) rather than with very hot water. They answer better when made with hot water (at the temperature above named) than with cold water, because they can thus be obtained in a more concentrated state. And seeing that in this kind of experiment the fluids have afterwards either to be boiled or otherwise superheated (before or after closure of the flasks), the slight increase in temperature during the preparation of infusions becomes of less consequence. But in studying Heterogenesis, and with the view *of witnessing all the higher changes* which may take place in a pellicle, the organic infusions or *macerations must be made with cold water*, and subsequently filtered.

size, and irregularly placed with regard to the direction of their long axis. Such areas are freely intermixed with other less altered portions in which the *Bacteria* are densely packed, even smaller than natural, and apparently not separated by any pellucid material. Any of the modified areas may after a time undergo changes, very similar to those which I have last described as resulting in the production of fungus-germs.

On the other hand, a totally different fate may occasionally await such modified areas. Thus, in a strong infusion prepared with water at a temperature of about 120°F, the pellicle was found to be abundant and pulpy; and on the second day areas of the kind above described were most marked and numerous[1]. The contained *Bacteria* very soon became notably larger and distinctly loculated—each loculus containing two or three granules; whilst the jelly-like material was so abundant that each *Bacterium*[2] was distinctly isolated from its fellows. These particular areas were watched for several days, and were not found to have any tendency to undergo segmentation, although myriads of *Monads* had been formed in adjacent portions of the pellicle, as well as Fungus-germs which had vegetated into mycelial filaments and bore numerous heads of spores, similar to those of a small variety of *Penicillium glaucum*. The *Bacteria* included within these areas seemed to possess too

[1] The daily atmospheric temperature being about 62°F.

[2] The corpuscular appearance of some of these bodies was so marked that they might, perhaps, more appropriately be spoken of as *Torulæ*.

much inherent vigour to lose their own individuality—a supposition which was confirmed by their great increase in size and subsequent development. On the fourth and fifth days many were seen which had grown out into minute filaments, resembling what is commonly regarded as *Leptothrix*, although they also possessed all the characteristics of a miniature fungus-mycelium.

Thus, then, we may have modified areas in which the contained units flourish and grow, whilst still preserving their own individuality; or we may have pellucid areas persisting as such for a certain time, whose units at last undergo a process of molecular fusion and regeneration, leading to the production of a segmenting embryonal area from which brown fungus-germs are produced[1]. And, lastly, there may be pellucid areas which, almost as soon as they are formed, begin to undergo those changes whereby they are converted into true embryonal areas.

[1] During this process the contained *Bacteria* disappear, and a whitish refractive and homogeneous protoplasm is produced in the place of the jelly and its contained granules. If we turn to the account given of the origin of the 'germinal membrane' in the ova of higher animals, we may be struck by the similarity of the phenomena. Müller says (Baly's Translation, vol. i. p. 9):—'It appears, indeed, that the germinal membrane is formed by the attraction and aggregation of the globules of the yolk; but all parts developed in this germinal membrane are produced by solution of these globules, and conversion of them into a matter in which no elementary particles can be distinctly recognised, and of which the molecules must at any rate be beyond comparison more minute than the globules of the yolk and germinal membrane.' The subsequent development of plastodermic cells from this mass also agrees closely with what occurs in our embryonal areas. (See vol. i. p. 211, note 2.)

Many variations exist in the character of these areas in different cases, some of which I will now attempt to describe, as I have lately had an opportunity of watching numerous transitional conditions.

The pellicle which formed on a filtered maceration of hay during frosty weather (when the temperature of the room in which the infusion was kept was rarely above 55°F, and sometimes rather lower than this) presented changes of a most instructive character. On the third and fourth days the pellicle was still thin, although on microscopical examination all portions of it were found to be thickly dotted with embryonal areas. Nearly all of them were very small, though a few areas of medium size were intermixed [1]. The smallest were not more than $\frac{1}{4000}''$ of an inch in diameter, and these separated themselves from the pellicle as single corpuscles; slightly larger areas broke up into two or three corpuscles; and others, larger still, into 4–10 corpuscles. In most of these small areas, the corpuscles were formed with scarcely any appreciable alteration in the refractive index of the matter of which they were composed: this simply became individualized, so that the corpuscles separated from the surrounding pellicle and from their fellows, still presenting all the appearance of being portions of the pellicle, and exhibiting from 4–10 altered *Bacteria* in their substance. In some cases the products of segmentation soon developed into actual flagellated

[1] In these medium-sized areas segmentation was accompanied by the production of homogeneous and highly refractive protoplasm.

Monads in a manner presently to be described; whilst in others they seemed to remain for a longer period

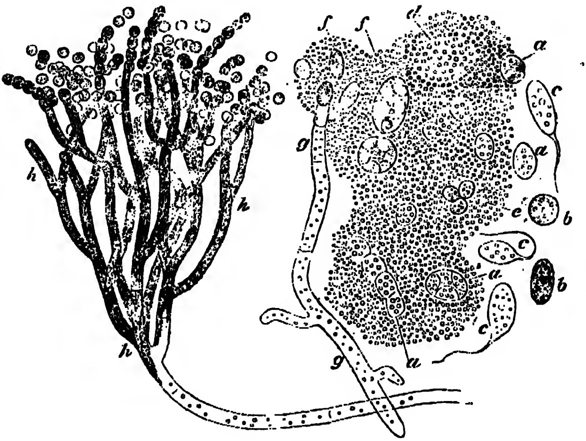


FIG. 56.

Simplest Mode of Development of Monads and Fungi from the Pellicle. (× 1670.)

a, *a*. First stage of differentiation of separate and aggregated corpuscles.
b, *b*. Such corpuscles in a more refractive condition, developing into Monads.
c, *c*. Fully-developed Monads.
d. Larger area in first stage of differentiation.
e. Refractive corpuscles which will develop either into Monads or Fungi.
f, *f*. More refractive corpuscles which give birth to mycelial filaments as in *g*, and ultimately expand into a form of *Penicillium* (*h*).

in the condition of simple motionless corpuscles. Other solitary corpuscles or small areas began to form in the pellicle in precisely the same manner, though they

speedily assumed a highly refractive and homogeneous appearance. Why some should undergo such a change, and not others, it seems quite impossible to say. One can only assert the fact, and add that these highly refractive ovoid corpuscles were, for the most part, more prone to produce Fungus-germs than Monads. Many of them soon grew out into dissepimented fungus filaments, which rapidly assumed the *Penicillium* mode of growth. The spores which were abundantly produced in terminal chaplet-like series were, however, small, homogeneous, spherical, and colourless.

On several occasions I have seen Monads produced in this way, by direct and immediate separation from the pellicle; though, as M. Pineau had stated, on other occasions they may be seen to arise in groups—in which they appear at first as aggregations of motionless corpuscles. The solitary mode of origin is that which has been described by M. Pouchet, and although the details given by him are not very full, so far as they go they are in accordance with my own observations. M. Pouchet, for instance, describes the flagellum as being closely applied to the body, and motionless for a time. This I have also found to be the case. I have, moreover, on one or two occasions been able to watch all the transitions from the mere motionless corpuscle to the flagellated Monad; just as, on other occasions, I have watched almost similar corpuscles develop into Fungus-germs.

Sometimes the flagellum is seen attached to cor-

puscles which still display almost unaltered *Bacteria* imbedded in their substance: generally, however, the corpuscles which separate from the pellicle in this comparatively unaltered condition, undergo certain slow changes before the flagellum is developed. The contained *Bacteria* become more and more indistinct, whilst the substance of the corpuscle grows rather more refractive and assumes the appearance of ordinary protoplasm. Corpuscles about $\frac{1}{5000}''$ in diameter are often very obscurely granular and quite motionless. They grow, however, and when they have attained the size of $\frac{1}{4000}''$ in diameter they frequently begin to exhibit slow undulating alterations in outline, and tend to assume an ellipsoidal form. One specimen $\frac{1}{3300}''$ in diameter, was seen without a flagellum, but slowly alternating between the spherical and ellipsoidal forms. Suddenly, at one extremity of the ellipsoid, a series of rapid contractions and protrusions of its substance were observed, and when they ceased, a motionless filament was seen bent around one side of the body. Three minutes afterwards a vacuole appeared for the first time at the opposite extremity of the ellipsoid. The corpuscle remained almost motionless for twenty-five minutes, merely exhibiting very slight changes in outline; after thirty minutes the first slow bendings of the flagellum were seen; and after thirty-five minutes the whole organism began to exhibit slow semi-rotations at intervals of a minute or two. After forty minutes the movements were pronounced and of a startling

character, dependent upon sudden contractions of portions of the body of the organism rather than upon movements of its flagellum. After fifty-five minutes the corpuscle unfortunately became hidden, owing to its having floated underneath a portion of the pellicle. How far the rapidity of the evolution of the flagellum, and its subsequent movements, were impaired by the glare of artificial light to which the organism was subjected, cannot be said. Certainly, however, the flagellum seems to be thrown out much more rapidly in other cases. Speaking of simple organisms of this kind, Dr. T. R. Lewis says[1]:—'Frequently a succession of pseudopodia are seen projected in a wave-like manner, as if lashing the fluid.' And again of other similarly active animalcules he says:—'Sometimes one flagellum is seen, a posterior one; at others, an anterior one also, both being retractile at will; and another may be darted forth out of any portion of its body.' Again, where tailed 'zoospores' are produced from Algæ, or from such Fungi as *Achlya* and *Cystopus*, they are also evolved most rapidly—two hours often sufficing for the entire production of a brood of such flagellated Monads from the segmentation of a mass of formless protoplasm.

Monads, indeed, are frequently produced from the 'pellicle' in precisely the same manner as that by which they arise within the terminal chambers of certain

[1] 'Report on the Microscopic Objects found in Cholera Evacuations, &c.' Calcutta, 1870, pp. 33 and 26.

Algæ or Fungi—that is to say, they result from the segmentation of a mass of homogeneous protoplasm[1]. The steps of this process we will now describe.

An infusion of hay was made with water at a temperature of 120°-130°F, and maintained at this heat for three hours. After filtration, about five ounces of the fluid were poured into a wide-mouthed bottle, and placed under a small bell-jar. When the fluid was examined at the expiration of three days, it was found to be quite turbid, and covered by a moderately thick pellicle. On removing portions of this pellicle and submitting it to microscopical examination, the fluid around was found to contain multitudes of very active specimens of *Monas lens*, having an average length of $\frac{1}{3300}''$, whilst the pellicle itself was mostly composed of medium-sized *Bacteria*, though there were a few areas of different dimensions in which the units had more the appearance of embryo *Torulæ*[2]. But, contrasting with the very pale fawn colour of the evenly granular pellicle, there were numerous refractive, and more or less homogeneous areas of a whitish colour. These areas

[1] In both cases, also, it happens that the products of segmentation are sometimes motionless and sometimes active units. We have already (p. 211) spoken of these differences as they are met with amongst derivatives of the pellicle. And on the other hand, as we have previously stated (vol. i. p. 182), the products of the subdivision of the encysted *Protomyxa*, or of the terminal dissepiments of *Achlya*, are actively moving bodies; whilst in the closely allied *Saprolegniæ*, in *Pezizæ*, and in other Fungi, the products of segmentation are perfectly motionless spores.

[2] Such as are represented in Fig. 48, *a*.

differed very much in shape and size—some were not more than $\frac{1}{1000}''$, whilst others were as much as $\frac{1}{150}''$

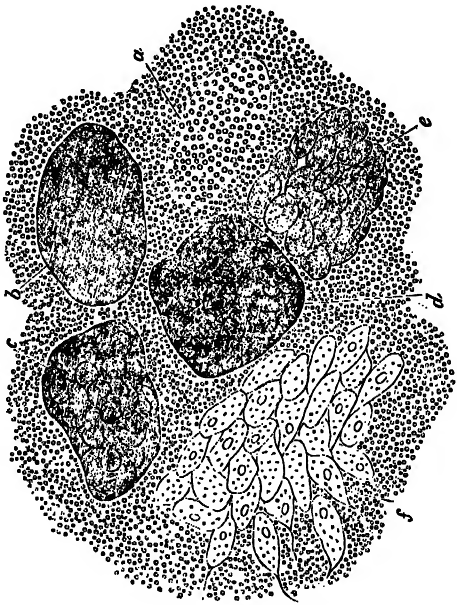


FIG. 57.

Segmentation of Embryonal Areas into Monads. (x 1670.)

a. First stage of differentiation.
b. Second stage—area almost homogeneous and refractive.
c. First traces of segmentation.
d. Segmentation more complete—units highly refractive.
e. Units less refractive—forming tailless corpuscles.
f. Fully-developed Monads derived from such corpuscles.

in diameter. Their shape was wholly irregular. Careful examination with a $\frac{1}{16}''$ and a $\frac{1}{32}''$ immersion ob-

jective made it easy to recognise such transitions as are depicted in Fig. 57. As in the instances previously recorded, the first appreciable stage in the formation of an embryonal area in the pellicle was a local increase in the amount of gelatinous material between the units of this portion of the pellicle, which thus became more distinctly separated from one another than in adjacent parts. Gradually these particles became less sharply defined, and at last scarcely visible, in the midst of a highly refractive protoplasmic mass which began to exhibit traces of segmentation.

Masses of this kind were seen, which had been resolved by such a process of segmentation into a number of spherical corpuscles about $\frac{1}{1500}''$ in diameter. These were at first highly refractive, though they gradually became rather less so, and revealed the presence of two or three minute granules in their interior. In other adjacent areas, a number of densely packed, pliant, and slightly larger corpuscles were seen actively pushing against one another. When they separated they were found to be active ovoid specimens of *Monas lens* about $\frac{1}{3300}''$ in length, and provided with a vacuole and a rapidly-lashing flagellum. On the fourth day the number of embryonal areas throughout the pellicle had increased, and the specimens of *Monas* existed in myriads in the infusion. They were tolerably uniform in size, though some were notably smaller than the average, owing to the fact that they were products of a recent fission, all the stages of which

were watched on many occasions[1]. On the sixth day many of the Monads had much increased in size, some of the larger of them measuring $\frac{1}{1800}''$ in length. Others had lost their flagellum, and were existing in the form of ovoid or rounded corpuscles, which were motionless, though still provided with a vacuole, and now also with a solid nucleus about $\frac{1}{20000}''$ in diameter (Fig. 58, *b*, *c*, *d*). All stages were seen, between the ovoid corpuscle $\frac{1}{3300}''$ in length and a much larger Amœba of the kind just described, which was either motionless or else, at intervals, exhibited slowly-evolved and blunt protrusions from its periphyry.

In other specimens the most easy and rapid alternations were seen between the shape and mode of locomotion which pertains to Monads and those which are characteristic of Amœbæ. Monads which had been previously in active motion would at times come to a state of rest, develop two or three vacuoles in their interior, and behave in all respects like an Amœba, save for the presence of the now languidly moving flagellum. After remaining in this state for a variable time, some of them would just as abruptly cease to display the amœboid movements, the extra vacuoles would disappear, the shape of the Monad would be resumed—and with it the lashing movements of the flagellum which again gives rise to the rapidly-darting

[1] It took place mostly in a longitudinal, though occasionally in a transverse direction. I have never seen the whole process occupy less than twenty minutes.

gyrations of the organism. Whilst in the amœboid state the changes in shape were moderately rapid; though two or three organisms were watched, one portion of which remained rounded and apparently attached to the glass, whilst the opposite extremity threw out and retracted comparatively long processes with lightning-like rapidity—some of them being filiform, like the ordinary persistent flagellum[1].

On the seventh day thousands of the motionless spheroidal Amœbæ were seen, which had much increased in size. They were now as much as $\frac{1}{1650}''$ in diameter, and displayed one or more vacuoles (Fig. 58, *d*). Each one contained a distinct nuclear particle, though there was an almost complete absence of granules—the body substance being quite pellucid. Some organisms of the same kind, though rather smaller, contained the ordinary granules in their interior and also exhibited slow amœboid movements; whilst many Monads of the same size and general appearance were seen exhibiting amœboid changes of form, though they had not yet lost their almost motionless flagellum.

On the eighth day there were myriads of active Amœbæ around every portion of the pellicle which was examined—they were in fact, at this period, almost as numerous as the Monads. Great numbers also existed in the spherical motionless condition.

[1] The rapidity with which such processes were emitted was similar to what was noticed at p. 214.

On the ninth day the pellicle began to assume a brownish colour on the surface, owing to the enormous

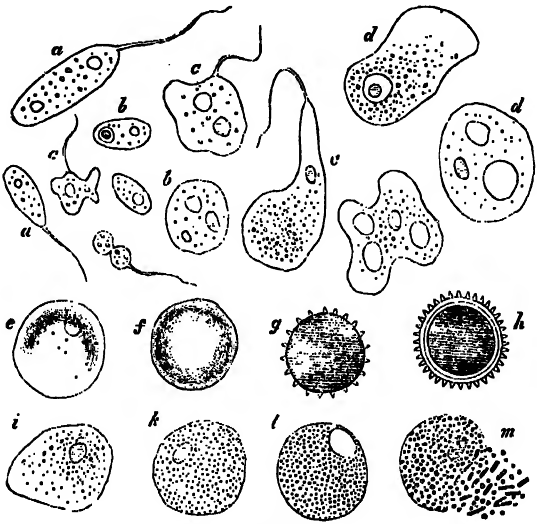


FIG. 58.

Phases in the Life-history of Monads and Amœbæ. (× 1670.)

a, a. Monads in different stages of growth.
b, b. Similar Monads which have lost or retracted their flagella.
c, c. Monads about to be transformed into Amœbæ.
d, d. Resulting Amœbæ in active and motionless stages.
e, f, g, h. Stages by which motionless Amœbæ become encysted.
i, k, l, m. Stages by which other Amœbæ become resolved into Bacteria.

development of minute, brown fungus-germs[1]. Portions of the pellicle were also separating and beginning to sink, whilst many of the spherical Amœbæ were undergoing changes destined to result in encystment.

[1] Very similar to those represented in Fig. 59, *e*.

On the tenth day, similar though more advanced changes were seen. Although the majority of the Amœbæ were still active and polymorphic, hundreds of them were becoming encysted, and the different stages of the process could be well seen. They were these:—The previously spherical Amœbæ lost their vacuoles, the granules almost wholly disappeared, and the body generally became slightly refractive—the nucleus being still visible. After a time the nucleus became invisible, and the whole substance of the organism assumed a homogeneous and highly refractive appearance—so that when it was examined a little beyond the focal distance it looked almost like an oil globule. There was a decided condensation, also, of the outer layer of protoplasm, this being the first trace of the cyst-wall. Subsequently the cyst-wall became more and more manifest, whilst the size of the sphere slightly diminished, and assumed a faintly brownish tinge. From the surface of the developing cyst there were a number of very short, ray-like projections (Fig. 58, *g*). In the last stage, whilst the cyst-wall became more developed and the projections more obvious, the whole exterior envelope assumed a decidedly brown colour, and the contained protoplasmic mass, which had again become less refractive, distinctly separated from the cyst-wall[1].

[1] In the course of the next few days myriads of the Amœbæ had undergone this kind of change, which is inevitable as soon as the activity of their vital processes becomes diminished. It is the extraordinary molecular activity and constant change of shape of the Amœbæ which tends to prevent the earlier occurrence of this primary differentiation.

As the virtues of this infusion seemed to be getting exhausted, on the same (tenth) day I transferred a portion of the pellicle to the surface of a new weak infusion of hay, which had been previously boiled. On the following day the Monads were found to have increased very much in size, and so also had many of the Amœbæ. Several large ovoid Monads on measurement were found to be as much as $\frac{1}{1000}''$ in length—they had, in fact, become nearly twice as long as the largest of those which had existed in the old infusion.

Five days afterwards (sixteenth day), when another portion of this transferred pellicle was examined, all the Monads were found to have disappeared, with the exception of a few which were in a motionless state and were apparently about to be converted into Amœbæ. These latter organisms existed in teeming myriads: a portion of them had become encysted, whilst of the rest about one half were active, and the others, though not encysted, were almost motionless and more or less granular. On further examination, it was found that the granular Amœbæ (Fig. 58, *i–m*) were organisms in a dying state, and that the contained particles were new living units which gradually developed into *Bacteria*. All the stages of this development were to be seen. Thus there were a considerable number of languid Amœbæ which merely displayed a slight increase in the customary number of minute particles situated near or around the nucleus. There were others in which these minute granules were more numerous; and others still,

quite motionless and spherical, which were densely packed with minute particles throughout their whole substance—these particles being motionless and less than $\frac{1}{100000}''$ in diameter. In many of such Amœbæ a clear vacuole was still to be seen. In other organisms the particles were of a slightly larger size, and owing to the protoplasmic substance in which they had been produced having become fluid, these particles were to be seen in active movement within an attenuated film which constituted the outer layer of the old organism whose nucleus was still visible. When reduced to this condition, trembling movements of the whole mass were seen, owing to the combined agitations produced by the contained units. Soon the attenuated outer membrane gave way, and as the contained units were liberated, they at once exhibited very active movements of progression, after the fashion of minute *Bacteria*. The surrounding fluid was, in fact, crowded with similarly minute and active *Bacteria*, and with others slightly larger, which had evidently been produced in this manner.

Such was the fate that overtook those Amœbæ which lived latest in the solution. Changes of an unhealthy nature seemed to have been so suddenly induced that the organisms did not possess sufficient energy even to undergo the process of encystment. Their own molecular movements (those which pertain to the ordinary life of the Amœbæ) being so languid, other retrograde changes were initiated, leading to the birth of new particles throughout their substance. *Bacteria*, in fact,

were generated by a most typical process of Heterogenetic Bioparadosis[1].

The changes which have been thus observed constitute a very remarkable series. The simplest living units (*Bacteria*) first swarm in the infusion; these become aggregated at the surface so as to form a 'proligerous pellicle,' in which embryonal areas gradually appear; as a result of segmentation in these embryonal areas, specimens of *Monas lens*, $\frac{1}{3300}''$ in diameter, more or less suddenly make their appearance; they increase in size, occasionally assume an amœboid appearance for a time, and are ultimately transformed into real Amœbæ. The transition is effected by the loss of the flagellum, the appearance of vacuoles in their interior, and the simultaneous manifestation of polymorphism and a creeping mode of progression; at the same time a nuclear corpuscle develops in the interior, and the whole animal grows considerably. At last the Amœbæ gradually cease to exhibit their characteristic movements, whilst they become more or less spherical and motionless. Ultimately a firm bounding membrane is produced, and they pass into the encysted condition, in which, although slightly smaller in size, they constitute spherules $\frac{1}{1650}''$ in diameter. On the removal of some of this pellicle to the surface of a fresh infusion, the Monads and Amœbæ greatly increased in size; all the Monads gradually became converted into Amœbæ, and some of these at first went through the ordinary

[1] See vol. i. p. 234.

process of encystment, though at last (on account of some more sudden change in the fluid) they seemed suddenly to lapse into a morbid state. They were apparently unable to encyst themselves, and not being capable of continuing as Amœbæ, there sprang up in their interior a teeming progeny of new units (*Bacteria*), the production of which occasioned the final dissolution of the organisms in which they were evolved.

Other changes, however, took place in this same infusion which deserve to be chronicled. On the sixth day there were seen scattered throughout those portions of the pellicle intervening between the embryonal areas a multitude of solitary spherules, varying in size from mere specks $\frac{1}{30000}''$ in diameter, or less, to bodies $\frac{1}{5000}''$ in diameter. They were colourless, quite motionless, and appeared to be solid and almost homogeneous masses of plasma rather than vesicular bodies (Fig. 59, *a*). There were merely faint indications of granules in their interior, and no evidence of a differentiated outer membrane. None of them seemed to be undergoing processes of self-division, and each appeared to have grown up in the situation in which it was seen[1]. These corpuscles gradually became more numerous on to the tenth day, though they underwent no appreciable

[1] These bodies were evidently quite different from *Monas* and its amœboid derivatives, all of which shrivelled very much when mounted in glycerine-jelly, though the corpuscles which I have just been describing underwent no change of this kind.

change except a slight increase in size. On the eleventh day, in the portion of the pellicle which had been transferred to the fresh hay infusion, many of these stationary bodies, like the Monads and active Amœbæ, were found to have increased to such an extent as to have doubled their transverse measurement. They had also developed a distinct nucleus in their interior (of a ring-like character); vacuoles appeared and disappeared at intervals; and at the same time they exhibited very slow and slight amœboid changes in outline (*g*, *h*, *i*). They were, in fact, now obviously converted into sluggish Amœbæ. On the seventeenth day many of them were recognised in the pellicle, scattered amongst the already-described encysted Amœbæ. They had again become motionless and slightly contracted in dimensions; whilst their outer layer was condensed, but not decidedly cyst-like. Many of the smaller sizes were also seen. Seven days afterwards (twenty-fifth day), when another portion of the transferred pellicle was examined, it was found to be densely studded throughout with thousands of encysted Amœbæ, the great majority of which were of the first variety and were pretty uniform in size and appearance. But interspersed amongst them were a considerable number of the imperfectly-encysted Amœbæ, of different sizes. Here and there, however, some of them—now mostly about $\frac{1}{1500}''$ in diameter—presented an unusual appearance. They had assumed a faint brown hue throughout their whole mass, and segmentation had gone on within

so as to produce a number of units, whose shape seemed irregular owing to their being so densely packed. Other

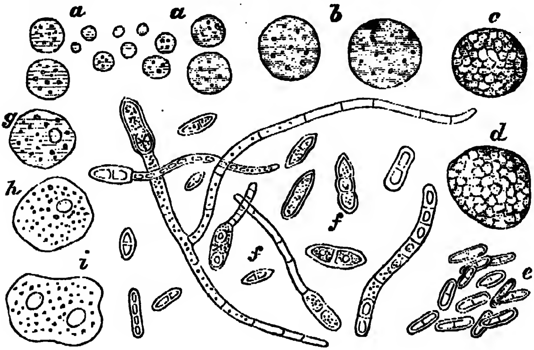


FIG. 59.

Similar Organisms segmenting into brown Fungus-germs or growing into Amœbæ. (× 1670.)

a, a. Motionless corpuscles of various sizes.
b. Similar corpuscles much increased in size.
c, d. Segmentation of such corpuscles into brown Fungus-germs.
e. Appearance of germs when liberated.
f, f. Development of the almost similar germs represented in *Fig.* 48, *d.*
g, h, i. Gradual conversion of other corpuscles, when transferred to another fluid, into Amœbæ.

masses were seen in which considerable growth had taken place—these being nearly twice the size and irregular in outline, though still of a faint brown colour, and composed of a mass of densely packed units, which were held together by an almost invisible bounding membrane. And, lastly, in other places small aggregations of brownish germs were seen, which had been liberated by the solution of this very attenu-

ated membrane—the separate germs being tolerably thick-walled, bilocular bodies about $\frac{1}{4000}''$ in length by $\frac{1}{12000}''$ in breadth. An examination on subsequent days showed many other of the amœboid bodies breaking up in a similar manner into these brownish, biloculated Fungus-germs.

But, strange to say, brown Fungus-germs of an almost similar character had previously presented themselves on the surface of the original infusion, although they had arisen in quite a different manner, and apparently by a process of Archebiosis.

In the original infusion, when the Amœbæ commenced encysting themselves (on the tenth day), portions of the pellicle began to sink to the bottom of the vessel. Three or four days later it was found that the portions of the surface of the fluid which had thus been left uncovered, were coated by a delicate, brownish film, which, when examined microscopically, displayed appearances similar to those represented in Fig. 48, *d*. An almost invisible and thin gelatinous stratum existed (a kind of formative membrane), in which every intermediate stage could be detected, between the most minute particle and a brownish, thick-walled, biloculated fungus-germ. The smaller bodies were colourless, solid-looking, and highly refractive; and they seemed much more like mere dead concretions[1] than living

[1] Such as are represented in Fig. 43; or such as appear in some ammonic tartrate solutions, and which are so closely allied to *Sarcina*. (See *Appendix* A, p. iv. Fig. *a*.)

things. All were motionless. Gradually, however, they became less refractive, grew more and more vesicular, and at last assumed a faint brown tint. Although most of them remained as bilocular bodies, others grew and underwent further segmentation, so as to produce tri- and quadrilocular bodies, or 'septate spores.' During all stages of growth, some of them seemed to undergo an occasional process of fission. They were watched for many days, but as the germs displayed no tendency to develop[1], some of them were immersed in a little syrup upon a glass slip, protected by a covering glass, and then set aside in a damp, air-tight, developmental chamber. After about ten days the germs were found to have become more colourless, to have budded and multiplied, and in many cases to have formed elegant mycelial filaments, such as are represented in Fig. 59, *f*, *f*.

These latter observations are interesting in many respects. It is remarkable, for instance, that germs of precisely the same appearance should arise after such different methods—by origin and growth in a formative membrane in one case, and as the result of the segmentation of a partially encysted Amœba in another case. Then, again, it is extremely interesting to find that these parental Amœbæ had, to all appearance, arisen by a process of Archebiosis, although at one

[1] This has been very frequently observed on other occasions. See p. 233.

stage of their development they were almost indistinguishable from other Amœbæ seen in the same infusion, which had resulted immediately from the transformation of flagellated Monads, and mediately as products of a process of segmentation occurring in an embryonal area. So that whether we have to do with Fungus-germs or with Amœbæ, their forms are occasionally so intimately associated with the matter from which they have been derived, that similarity may ultimately be met with between organisms whose actual modes of origin have been most diverse.

These amœboid corpuscles which grew up in the midst of the pellicle were peculiar in many respects. In their very early stages it was quite impossible to say whether they were going to develop into Fungus-germs or into Amœbæ; ultimately, however, they seemed to lean more towards the latter mode of development, although the activity which they displayed in this phase of their existence was extremely slight. Finally, we find them, after encystment, undergoing a process of segmentation, by which they give rise to a colony of brown Fungus-germs, in precisely the same manner as that by which the *Protomyxa* of Haeckel gives origin to flagellated Monads which subsequently assume the characters and mode of locomotion of Amœbæ[1]. This evidence,

[1] Just as Amœbæ may arise either by Archebiosis or by segmentation of pre-existing living matter (in embryonal areas), with or without passing through the Monad phase of existence, so may Fungus-spores arise by either of these methods. There is also much evidence to show that Monads may arise directly by a process of Archebiosis. I have

combined with what has been already alluded to in Chapter XV, and in addition to other facts previously known, tends to show that the transition from the Amœba to the Monad, or the reverse, may be paralleled by a similar interchangeability between the form and mode of growth of an Amœba and that peculiar to a Fungus—so that either form may at times result from one and the same living matter when it undergoes internal modifications, with or without being subjected to new conditions. This position is still further strengthened by the facts which I have now to record.

A few days after having made the infusion, the changes in which have just been described, I prepared another with a portion of the same sample of hay. This second infusion, however, was made with water at a temperature of 158°F, which was maintained at this heat for two hours. After filtration it was placed in a similar vessel, and allowed to stand side by side with the other infusion. On the third day, embryonal areas of various shapes and sizes were seen in the firm pellicle which had formed upon the surface[1].

on one or two occasions seen small Monads tolerably abundant in infusions of hay which had only been prepared twenty-four hours, and in which no coherent pellicle had yet formed in which they could have arisen by the secondary process. Moreover, they have been found in sealed flasks in which no pellicle was present, even by M. Pasteur. In my own *Experiment b* (vol. i. p. 443), the Monads must have had this primary mode of origin. Here some of the new-born specks of living matter seemed to have grown into Fungus-germs, some into minute Amœbæ, and others into Monads.

[1] The daily temperature being about 60°F.

These areas were distinguished by their whitish, refractive appearance from the slightly fawn colour of the contiguous unaltered pellicle. Particles of some kind were obscurely seen within the refractive protoplasm, and on the following day many of the areas, which had increased in number, showed signs of commencing segmentation. This process went on com-

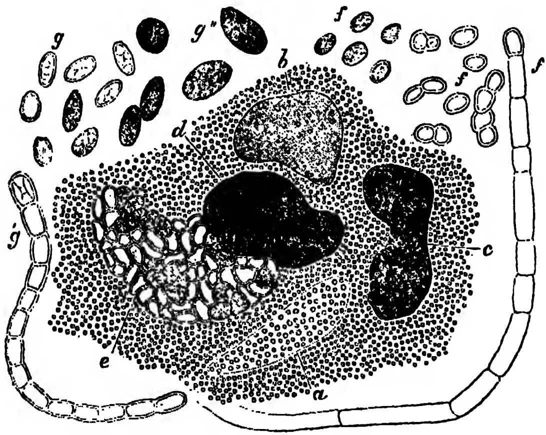


Fig. 60.

Segmentation of Embryonal Areas into Fungus-germs. (× 1670.)

a. First stage of differentiation.
b. Area almost homogeneous and refractive.
c. First stage of segmentation.
d. Area showing more complete segmentation.
e. Area in which homogeneous refractive products are being converted into brownish vesicular Fungus-germs.
f, f′. One form of germ in different stages of development.
g, g′. Another form of germ in different stages of development.

paratively slowly, and two or three days elapsed before the segmentation was completed. But at last some of the areas were wholly resolved into a number of colourless, homogeneous, and highly refractive spherules, about $\frac{1}{8000}''$ in diameter. Some areas seemed to remain in this condition for two or three days longer, whilst in others the products of segmentation began to undergo change almost before it was completed. In each case, however, the modification was of the same kind, and consisted in a gradual diminution in the refractiveness of the separated elements, and their assumption of a more distinctly vesicular character, whilst they simultaneously acquired a faint brown colour. They were thus converted into unmistakeable fungus-germs, although they showed very little tendency to germinate; and it was not until after repeated examination that a few of them were found growing out into filaments such as are represented in the figure. Occasionally, in the same pellicle, the embryonal areas broke up into products of a somewhat different character. The segments were slightly larger, whilst they gradually assumed a deeper brown colour and a more compound character. These elements also grew at this stage, and underwent processes of division after the fashion of Lichen gonidia, and in a manner similar to what I had observed on a previous occasion[1]. These germs also exhibited very little tendency to develop, though on one or two

[1] See p. 203, Fig. 55.

occasions they were seen to have grown out into short, chain-like filaments, such as are represented in Fig. 60.

During all the period in which the embryonal areas were breaking up into these corpuscles, which soon assumed the form of brown Fungus-germs, not a single Monad or Amœba was to be seen in the solution—and yet during the whole time it had been standing side by side with the other infusion which was prepared at a temperature of 120°–125°F. Facts of this kind have been observed on several other occasions with great constancy, so that one may safely state that Fungus-germs or Monads and Amœbæ may be procured at will, by simply regulating the amount of heat at which the infusion is prepared. The Monad and the Amœba represent more animalised modes of existence, which are only able to manifest themselves in infusions in which the organic matter has not been too much deteriorated by the influence of heat. Such deterioration seems to manifest itself by altering the developmental potentialities of the primary forms of living matter evolved in the infusion[1].

[1] Seeing that the Monads or the Fungus-spores are produced, not from invisible germs but from the segmentation of large embryonal areas, every stage of whose formation can be accurately traced, this seems the only possible explanation. If the opponent of Evolution contends, in answer to one set of experiments with heated fluids and closed flasks, that Monads are met with because their germs are capable of resisting a temperature of 266°F, he cannot now contradict himself by saying that embryonal areas formed on infusions which have been prepared at a temperature of 158°F do not yield Monads, because such a temperature is destructive to their germs. Neither is it open to him to say

Experience has shown me that if an infusion has been heated for a time to 212°F, the pellicle which forms on its surface very frequently never gives rise to embryonal areas; if the infusion has been prepared at a temperature of 149°-158°F, the embryonal areas which form will give origin to Fungus-germs; whilst in a similar infusion prepared at 120°–130° F, the embryonal areas, which seem at first to be in all respects similar, break up into actively-moving Monads. It remains for us to see what changes may take place in a pellicle which forms on an infusion or maceration prepared with cold water (60°–70°F).

Before passing to a description of these phenomena, however, I will describe the mode of origin of the embryos of some organism whose real nature is unknown—the final stages of its development not having been traced. As far as they were seen, the stages were of a very positive character.

I have observed these early stages in two different infusions; but in each case, after a certain stage of development had been achieved, no further progress seemed to be made for about two days, and then the pellicle unfortunately broke up and sank to the bottom. The arrest of development may therefore have been

that embryonal areas yielding Fungus-germs do not appear in infusions prepared at 212° F, because such heat is destructive to them; when at the same time he vehemently contends, in answer to other experiments, that similar Fungus-germs are not hindered from developing after exposure to such a temperature, or to others which are much higher.

due in each case to some morbid quality of the pellicle itself. These organisms were observed in the middle of the month of April (1869), in an infusion of turnip leaves, which had been prepared fourteen days previously. All stages of development could be seen in different parts of the pellicle. The new organism first manifested itself by the presence (in a uniformly granular layer) of an aggregation of 8–20 larger and more refractive particles, which gradually became marked off from the surrounding granules by a thin but distinct bounding membrane. The granules continued to in-

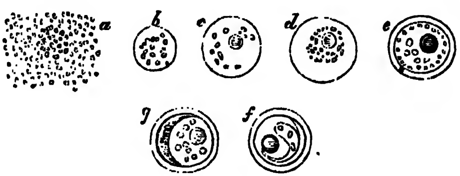


Fig. 61.

Mode of Origin and Development of an Embryo of uncertain nature. (x 800.)

crease in size; and at a later stage the containing sphere was seen to have grown larger, whilst the granules had assumed a crescentic arrangement (*c*). On their concave side there was a tolerably large refractive globule about $\frac{1}{12000}''$ in diameter, which exhibited the most distinct oscillations and more or less extensive to and fro movements in the otherwise clear central space. In other specimens this central globule had become even larger and the granules had closed round

it more equally, so as to leave a broad space between the central mass and the thin walls of the containing sphere. The measurements in this stage were found to be as follows:—containing sphere $\frac{1}{2000}''$, central nuclear-like body $\frac{1}{10000}''$, and surrounding mass of granules $\frac{1}{5000}''$ in diameter. Afterwards the central nuclear-like body and the granular mass seemed to become lighter in colour—the former still exhibiting its slow oscillating movements, whilst the latter had much increased in size so as more nearly to fill the delicate cyst in which it was contained (*e*). Then the outlines of the embryo gradually became more defined; three or four other rather large granules appeared in the neighbourhood of the nucleus, and one crescentic portion of the embryo-mass presented a smooth, glistening, and homogeneous appearance. No later stages were traced; and though no movements of the embryo as a whole were seen—only movements of the nucleus—there could not be the shadow of a doubt that these bodies represented organisms of some kind, which were developing, not from ova, but as a result of changes taking place in the very substance of the pellicle itself.

The intermediate connecting links between the Flagellated Monads on the one hand, and such Ciliated Infusoria as *Paramecium* and *Kolpoda* on the other, are undoubtedly such forms as those which were included by Dujardin in his genus *Enchelys*. They are scarcely larger than many Monads; they possess the same simple

structure, having no trace of an oral aperture, though like the Monads they display an internal vacuole, and

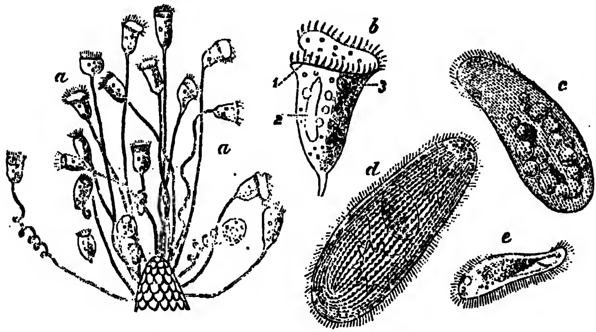


FIG. 62.

Ciliated Infusoria.

a. Group of *Vorticellæ*. *b.* Single *Vorticella*, more highly magnified. *c. Kolpoda.* *d. Paramecium.* *e. Enchelys.*

like them also they may or may not possess a simple nuclear particle. They present the same variations in form which are to be met with amongst Monads, and they differ from them only by the possession of vibratile cilia over most of their body, instead of possessing one much longer flagellum. They are, moreover, not unfrequently met with in large numbers in situations in which Monads abound.

Pineau says he has watched the development of organisms of this kind in a pellicle which formed on an infusion of isinglass. The first stages were altogether similar to those which he has described as having

taken place in the evolution of *Monas lens*[1]. Corpuscles were seen to separate from the embryonic aggregations without a flagellum, though they continued to increase in size, and soon developed a vacuole and nuclear particle in their interior. As they enlarged they gradually assumed an oval form, though still remaining motionless and devoid of cilia. At last, with very little further increase in size, cilia were developed[2], and the organisms gradually displayed the appearance and locomotory powers which have been attributed by Dujardin to the form which he named *Enchelys ovata*[3].

The organisms previously mentioned have nearly all been minute, and it has therefore been somewhat difficult to trace their early stages. These difficulties, however, gradually vanish when we come to the investigation of the mode of origin of such larger organisms as *Paramecia* and *Kolpoda*. Although their most remarkable mode of origin was fully described and figured by M. Pouchet more than twelve years ago, yet, unfortunately, many of our leading biologists have preferred to repudiate his statements, and rely upon their own

[1] See p. 196.

[2] The apparition of cilia is known to be quite sudden in the development of the spore of *Vaucheria*, and also to be sudden during the development of other Infusoria, such as Cienkowski and others have observed. (See *Appendix* D, p. xciv. note 3.)

[3] To another similar solution of isinglass M. Pineau, mindful of the results recorded by Dutrochet, added a few drops of vinegar, and he says:—'Il ne s'y developpa un seul animalcule: mais en revanche elle se couvrit, comme je m'y attendais, d'un forêt de moisissures.'

notions concerning credibility and the mode in which living matter ought to conduct itself, rather than adequately investigate the subject for themselves.

According to Pouchet, the stages in the evolution of *Paramecium viride* were as follows:—The pellicle which was at first uniform and evenly granular, after a short

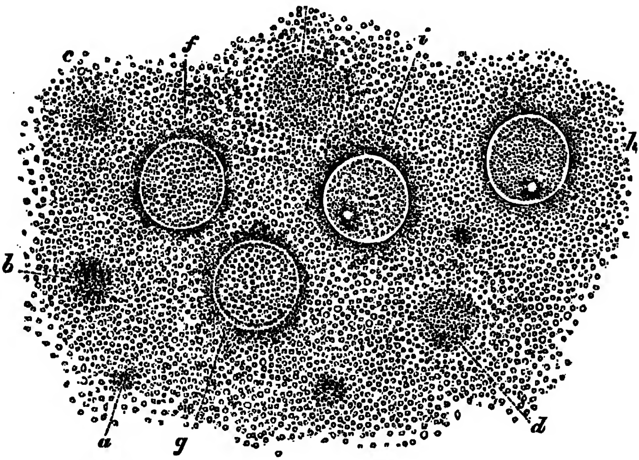


Fig. 63.

Mode of Origin of *Paramecia* from the 'Pellicle,' after Pouchet. (× 400.)

time changed in aspect here and there, owing to a concentration of its granules at tolerably equal distances into small more or less rounded aggregations which soon became surrounded and defined by a clear border, suggestive of a resemblance to the *zona pellucida* of higher animals. The next change which took

place was that the granules, which had been at first more densely aggregated towards the centre, disseminated themselves uniformly through the ovum—whilst at the same time the simple clear zone thickened into a distinct membrane. At this stage the whole egg appeared somewhat lighter and more transparent than the surrounding pellicle. Soon after this—differentiation still proceeding—the mass of enclosed granules gradually became converted into a real embryo, which manifested its existence by slow movements—at first by simple oscillations in the mass of granules, and then by regular uniform movements of revolution of the whole contents within its enveloping membrane, similar to those of many other embryos. The slightest shock at this stage immediately arrested the gyration. Then, after a time, a pale spot appeared amongst the granules in some part of the embryo, the alternate contraction and dilatation of which soon showed that it was the contractile vesicle of the infusorium. After a time the embryo began to exhibit movements of quite a different kind—sudden and irregular—no longer checked, but rather increased by slight shocks from without. In one of these sudden plunges the thin enveloping membrane was ruptured, and there entered into the aquatic world a free-swimming and ciliated infusorial animalcule having the characteristics of the species above mentioned.

Such is the marvellous story, and the description of other observers is substantially similar. In the particular

observation of which M. Pouchet gives the details[1], the first rudiments of the eggs began to make their appearance in the pellicle of an infusion of hay on the second day; on the third day the ovules were distinctly circumscribed, spherical, and $\frac{1}{600}''$ in diameter; on the fourth day there was no increase of size, the investing membrane could scarcely be recognised, although there was a distinct gyration of the embryo within it, and in those which were most advanced the contractile vesicle could already be discovered; on the fifth day the embryos were found to be of the same size, though slightly greenish in colour, and their movements were more irregular and jerking. At this stage the animalcule had assumed a pyriform shape, fine cilia could be seen on some parts of its surface, and the contractile vesicle was most obvious in the midst of minute and densely packed greenish granules. After a few hours more, the buccal cleft fringed with longer cilia became obvious, and also the so-called nucleus in the centre of the body. The embryos had by this time somewhat increased in size, so that after an interval of a few more hours fully developed specimens of *Paramecium viride*, $\frac{1}{450}''$ in diameter, were swimming about in the solution.

These observations of M. Pouchet have been repeated by him over and over again. He has thus seen different forms of *Paramecia* arise in the pellicle, and at other times, by steps essentially similar, *Kolpodæ* have

[1] 'Hétérogénie,' p. 394.

made their appearance. The difference between these two forms is indeed quite trivial and unimportant, and wholly unworthy, even from the old point of view, of being regarded as a generic mark of distinction[1].

These observations of M. Pouchet have been confirmed by MM. Joly and Musset, M. Pennetier and others. The former observers declare[2] that they have watched the evolution of specimens of *Kolpoda cucullus* in a pellicle that formed on water in which the contents of a hen's egg were allowed to macerate. In this pellicle there appeared, as they say, 'en vertu d'une sorte de cristallisation vitale,' the spherical masses of granules constituting 'les œufs spontanés' of Pouchet; and these in their turn, after a period in which the usual rotation of the embryos within the egg membrane was observed, gave origin to specimens of the organism above mentioned. On the removal of the first pellicle it was succeeded by another in which similar developmental phenomena were repeated.

[1] The actual disposition of the ciliæ in different specimens of Infusoria is subject to considerable variation. And yet many supposed species of *Oxytricha* were distinguished from one another by Ehrenberg according to the number and disposition of their ciliæ, though M. Haime says ('Ann. des Sc. Nat.' 1853, p. 117), 'la disposition de ces soies est sensiblement la même dans les diverses espèces du genre Oxytriche, ou du moins dans les diverses formes décrites comme telles.' Mr. Carter goes still further. He says ('Ann. of Nat. Hist.' 1859, p. 249) that Haime's *Oxytricha*, as well as Ehrenberg's *O. pellionella*, *Kerona polyporum*, and *Stylonichia silurus*, are 'only states of the extremely variable *Kerona pustulata*.'

[2] See 'Compt. Rend.' (1860), t. li. p. 934.

I have also myself, quite recently, watched with the greatest interest all the stages of this process, which terminated in the evolution of fine specimens of *Paramecia*, and am most pleased to be able to bear my testimony to the general accuracy of M. Pouchet's description. Up to this period I had never seen a single *Paramecium* or other specimen of the larger ciliated Infusoria in any of my hay infusions—these having all been prepared either with warm or with hot water. But about ten days previously, on re-reading M. Pouchet's description of the mode of evolution of these organisms, it struck me that I had failed to see these phenomena owing to my never having made any infusions with cold water. I therefore at once prepared such a maceration, and two or three days afterwards wrote to M. Pouchet on the subject. In the reply which he was kind enough to address to me he said:—'Jamais, jamais vous ne renconterez *un seul* infusoire cilié dans une expérience faite à l'eau chaude. . . . . Il faut pour cela opérer sur des macérations faites à froid; alors vous obtiendrez facilement la phenomène de développement des œufs spontanés des Paramécies, dans les membranes proligères qui se seront formées d'abord[1].'

On the evening of the day on which I received this letter I again examined the thick pellicle which had formed on the maceration of hay, and much to my

[1] M. Pouchet has been in the habit of using one part by weight of ordinary dry hay to about forty parts of water, and of letting the maceration stand for two or three hours before filtering off the clear liquid.

delight I found it studded with thousands of embryo *Paramecia*, whilst others were free and active in the infusion. It was, therefore, a most significant fact that they should have been met with on the very first occasion that a cold maceration had been employed[1]; whilst not a single *Paramecium* had ever been seen before in any of the many hay infusions kept in the same place[2], although several of them had even been made with water whose temperature was not more than 125°–130°F, and which therefore was not high enough to have killed any embryos that may have chanced to pre-exist in the infusion previous to its filtration.

The maceration was at the time covered by a thick pellicle, which had become brown on its upper surface. Its under layers, however, were still soft and pulpy. When a small portion of it was transferred to a microscope slip, and gently compressed by the covering glass so as to flatten it out into a thinner layer, the granular membrane was observed to be pretty thickly studded

[1] Owing to the coldness of the weather (the daily temperature of the room being scarcely above 60°F) they did not make their appearance in the pellicle till more than fourteen days; although with a daily temperature of 75°F they are said by M. Pouchet to begin to make their appearance on the third or fourth day. I had examined the pellicle of my maceration from time to time during the first week, but did not look at it subsequently for several days—not, in fact, until the day on which I received M. Pouchet's letter. During the first week the pellicle had become very thick and pulpy, but the weather being rather colder at this time, it was principally giving birth to various kinds of Fungus-germs.

[2] Beneath a bell-jar in my study.

with the most distinct, egg-like bodies[1], varying in size from $\frac{1}{800}''$ to $\frac{1}{350}''$ in diameter. What struck me more than anything was the extreme distinctness with which almost all the phenomena described by M. Pouchet were to be seen. There could be little room for doubt with such objects before one.

The only difficulty experienced was to make out the exact nature of the first change by which the egg-like body became differentiated from the surrounding substance of the pellicle. I laboured under some disadvantages from having to examine an old and somewhat opaque pellicle, but after the most careful and repeated observations with reference to this point, I have been led to adopt an opinion slightly different from that of M. Pouchet. Instead of small concentrations of granules occurring—which gradually increased in size and at last became enclosed by a bounding membrane—it seemed to me that the differentiation took place after a manner essentially similar to that by which an ordinary 'embryonal area' is formed[2]. The small embryos did not appear to represent the earlier stages of large embryos; and it seemed rather that spherical masses of the pellicle of different sizes began to undergo molecular changes, which terminated

[1] They were actually embedded in the very substance of the pellicle.

[2] On the other hand, the embryos of unknown organisms which were seen to form in the infusion of turnip leaves (see p. 236), did seem to develop in a manner remarkably similar to the embryos of *Paramecium viride*, as described by M. Pouchet.

in the production of *Paramecia* of a correspondingly different bulk. Just as in the previously described

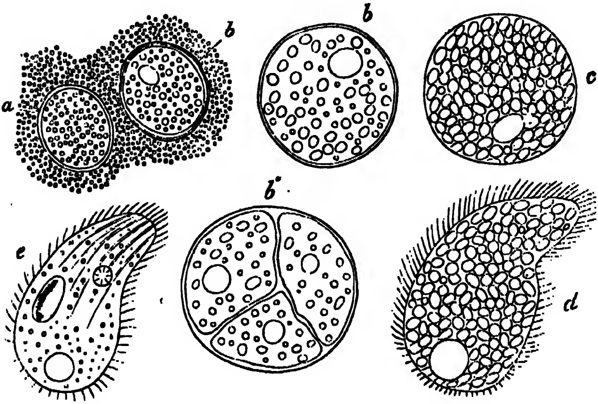


FIG. 64.

Mode of Origin of *Paramecia*. (X 800.)

a. First stage of differentiation.
b. Later stage in which vacuole has appeared.
b′. Similar stage of much larger embryo.
b″. Another embryo which has segmented into four (only three parts visible).
c. Later stage—embryo filled with large particles and revolving within its cyst.
d. *Paramecium* after it emerges from its cyst.
e. *Nassula*-like form into which many afterwards passed.

embryonal areas, masses of different size began to exhibit signs of change; so also here spherical portions of the pellicle, differing within the limits above mentioned,

began to undergo other heterogenetic changes. This was first indicated by an increased refractiveness of the area (especially when seen a little beyond the focal distance), and almost simultaneously a condensation of its outer layer seemed to take place, whereby the outline became sharply and evenly defined[1]. At this stage an actual membrane is scarcely appreciable, and the substance of the embryo (when examined at the right focal distance) scarcely differs in appearance from the granular pellicle of which it had previously formed part,

So far as it could be ascertained, the individual embryos did not increase in size, although they went through the following series of developmental changes. The contained matter became rather more refractive, and the number of granules within diminished considerably, whilst new particles after a time seemed gradually to appear in what was now a mass of contractile protoplasm. These new particles were at first sparingly scattered, though as they were evolved they continued to grow into biscuit-shaped particles, which sometimes attained the size of $\frac{1}{10000}''$. All sizes were distinguishable, and many of them moved slowly amongst one another, owing to the irregular contractions of the semi-fluid protoplasm in which they were

[1] The first changes seem to take place rather rapidly, judging from the great difficulty of recognising the earlier stages. It was almost impossible to find an area which was not already bounded by a delicate outer layer.

imbedded. Gradually the number of homogeneous, biscuit-shaped particles increased, and at last a large vacuole slowly appeared in some portion of the embryo. It lasted for about half a minute, disappeared, and then after a similar interval slowly reappeared. Much irregularity, however, was observed in this respect. The next change that occurred was the complete separation of the embryo from the cyst which it filled, and the commencement of slow axial rotations. These rotations gradually became more rapid, though they were not always in one direction. The embryo became more and more densely filled with the large biscuit-shaped particles, and at last the presence of cilia could be distinctly recognised on one portion of the revolving embryo. Then, as M. Pouchet stated, the movements grew more and more irregular and impulsive, so as at last to lead to the rupture of the thin wall of the cyst—when the embryo emerged as a ciliated and somewhat pear-shaped sac, provided with a large contractile vesicle at its posterior extremity.

Sometimes the embryo mass at an early stage of its evolution divided into two or four bodies, each of which developed within the cyst into a perfect embryo, and in place of exhibiting a regular rotation, they rolled and tumbled over one another in the most irregular manner. On one occasion I saw a cyst containing two embryos and four spherical Monads about $\frac{1}{5000}''$ in diameter, the latter having apparently

resulted from the fission of some smaller portion of the embryo mass. Sometimes it was the largest embryos which were observed to undergo this process of fission, though it was by no means confined to them[1].

On emerging from the cyst all the embryos, although differing somewhat in size, were of the same shape. This closely corresponded with the description given of *Paramecium colpoda* in Pritchard's 'Infusoria,' namely, 'Obovate, slightly compressed; ends obtuse, the anterior attenuated and slightly bent like a hook.' Cilia existed over the whole body, though they were largest and most numerous about the anterior extremity. No trace of an actual buccal cleft could be detected, and (except in the posterior portion of the body, where a large and very persistent vacuole was situated) the organism was everywhere densely packed with the large, homogeneous, biscuit-shaped particles. For many days these most active Infusoria seemed to undergo little change, though afterwards the number of the contained particles gradually began to diminish, whilst the body became more and more regularly ovoid, and a faint appearance of longitudinal striation manifested itself—more especially over its anterior half. At the

[1] Partial desiccation has a strong tendency to induce such fission, as I found by the frequency with which it occurred when the water had in great part evaporated from specimens placed in a developmental chamber. Fission of *Penicillium* filaments (into conidia), and of encysted *Euglenæ*, have several times been seen under similar circumstances.

same time a very faint and almost imperceptible mass ('nucleus') began to reveal itself near the centre of the organism, and when examined with a magnifying power of 1670 diameters, a lateral aperture (mouth) $\frac{1}{8000}''$ in diameter was seen which was fringed by short active cilia, arranged like the spokes of a wheel. These peculiarities correspond very closely with those of an embryo *Nassula*. Very many were seen with similar characters, and multitudes existed in all conditions intermediate between this stage and that of the simpler organism which first emerged from the cyst. No further stages, however, could be watched, as at this time some change took place in the infusion which proved fatal to all the free Infusoria and also to the multitudes of embryos which were at the time developing in the pellicle. These became more minutely granular and opaque, their movements ceased, and the cyst-wall grew thicker. This phase of development disappeared, therefore, almost as suddenly and mysteriously as it had appeared. The cysts were examined from time to time for many weeks afterwards, but they seemed to undergo no further change[1].

[1] In a maceration which was subsequently made during very cold weather, when the temperature of the room, even during the day, was rarely higher than 53°F, very large Amœbæ, some of which were $\frac{1}{180}''$ in diameter, and visible to the naked eye, were produced from the pulpy, under portions of the pellicle. They formed great masses of living, granular jelly of the simplest description—too large to move as a whole, though fluxes of portions of their semi-fluid body-substance were continually taking place in different directions.

With regard to the origin of other Infusoria, we may state that M. Pouchet speaks of the appearance of *Vorticellæ* in different infusions[1], though he does not give any detailed description of their mode of origin. He describes the cyst as being thicker, and sometimes of a bluish colour, and in his 'Hétérogénie' he represents some of the stages whereby a *Vorticella* of exceedingly simple structure is developed. M. Pineau, however, had previously described the mode of origin of organisms of this type. He saw them arise by a method that seems more closely to have resembled the mode by which Monads are produced than that by which the *Paramecia* appear. It is quite possible, however, that *Vorticellæ* may be evolved after different methods, just as we know that multiple modes of origin are met with in the case of Fungus-spores, Monads, and Amœbæ.

In a maceration of different plants, M. Pineau says, a granular pellicle appeared on the surface, which soon seemed to divide into spherical masses about $\frac{1}{2200}''$ in diameter. Some of the larger of these, after a time, exhibited motionless, ray-like processes. Elsewhere, globules of a similar character, though more distinct, were seen to have separated from one another. They were now all provided with processes, which, moreover, were seen to exhibit very slow movements, so that in this stage they resembled some varieties of the organisms usually called *Actinophrys*. As development pro-

[1] 'Nouvelles Expériences,' p. 245.

gressed, one of the previously equal rays attached itself to some neighbouring body, and then began to increase

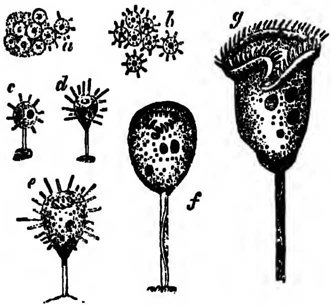


FIG. 65.

Mode of Origin and Development of *Vorticellæ*, after Pineau. (× 200.)

in size so as to constitute a sort of pedicle. In this form, according to Pineau, it resembled *Actinophrys pedicellata.* Other pediculated specimens were soon seen which had assumed a pyriform shape, and then the pedicle was no longer contractile, though the other rays still exhibited very slow movements. Some of these organisms presented the trace of a circular mouth at their free extremity, and specimens which had attained this grade of development seemed to increase in size, whilst the mouth became larger and was at last provided with a circle of vibratile cilia. As the organisms still further increased in size, the rays gradually disappeared, and the previously motionless pedicle also began

to manifest its contractility[1]. The organisms were then true *Vorticellæ*, though they subsequently still further increased in size, and gradually assumed the ordinary campanulate form.

Concerning these and the other observations recorded in the same paper, M. Pineau says:—'Tel est le résultat auquel je suis arrivé sur un des points le plus délicates de l'étude des êtres microscopiques, et sur lequel j'appelle l'attention des observateurs, avec d'autant plus de confiance, que ce n'est qu'après de nombreuses tentatives et des observations maintes fois répétées, que je suis arrivé à une entière certitude à ce sujet.'

How is it that such specific organisms can spring so sharply into existence, without any ordinary parents from whom they might have been supposed to inherit their specific characters? This is the principal difficulty which at present stares the evolutionist in the face. But let us not, on account of our present unfamiliarity with such possibilities, suppose the difficulty greater than it is. Let us contrast this unfamiliar problem with that with which we are more familiar, and whose difficulties, therefore, we are only too apt to gloss over. Is there not a somewhat similar difficulty with regard to the genesis of crystals out of solutions

[1] The originally non-contractile condition of the pedicle is quite in accordance with the observations of Stein and others.

in which their ingredients are contained? How can we imagine that such specific shapes as rhomboid dodecahedrons or octahedrons are enabled to appear *de novo* in homogeneous solutions? Have we not here facts of almost the same order? Familiarity with such occurrences, as actual and recognized phenomena, is apt to dim our mental vision, so that we pay no heed to what must have been the primary difficulty of the conception. Yet the fact that such definite and specific forms do spring up in solutions, whenever the suitable conditions exist, is doubted by none. The chemist explains it as well as he can. And if, in so doing, he is allowed, from the necessities of the case, to postulate the existence of inherent tendencies and molecular affinities to account for the particular forms assumed by his cohering molecules, why, by the name of all that is fair in Science, may not the biologist be allowed to resort to similar explanations as to the rationale of a process which, though at present unfamiliar to many, is nevertheless as much a matter of fact as the process of crystallization itself? Does not the chemist find that the form of a crystal, of any given substance, may vary according to the particular combination of conditions under which it is produced—and that within no very narrow limits? And when we have to do with matter of a higher, more complex, and more unstable character, is it not to be supposed that the limits of possible variation would increase in a more than proportionate ratio? So that from what we

know of the process of crystallization, we ought not only to be more easily convinced as to the possibility of the evolution of even specific organisms out of solutions containing organic matter, we might even have been prepared for the occurrence of all that marvellous interchangeability of diverse organic forms which we actually do encounter.

But let us take another illustration, showing that even biological phenomena with which we are quite familiar and which we are bound to accept, are nevertheless quite incapable of being understood.

We are perfectly familiar with the fact that the ovum of every animal tends to go through a uniform series of changes, and at last to give birth to the same kind of animal as that from which it was itself derived. But if, at the same time, we could place under the microscope a human ovum and others, at a similar early stage, of a monkey, a pig, and a sprat, how slight would be the differences presented. The kind of structure is perfectly similar in each case. Each has been formed as a free organic element, and though they may differ from one another only a little in point of size, yet how different are the ultimate products! The seemingly similar masses of granules are endowed with the most diverse potentialities—from the changes ensuing in one a reasoning man may be produced, whilst from those in another a man-like ape, a grovelling mammal, or an insignificant fish may appear. And yet such changes are nothing but the

concentrated results of those very tendencies to assume specific forms modifiable by circumstances, which we see manifested by the organisms met with in infusions. Had such tendencies been absent, the multitudinous forms of living matter could never have attained their present diversity and complexity. So that a belief in the existence of inherent tendencies to assume specific forms, in the lowest organisms, is as necessary for the biologist as a belief in the 'Law of Heredity'—whereby the potentiality of assuming any acquired specific form, however complex, is bequeathed to the germ of each organism.

In the case of the evolution of the ciliated infusorial animalcule, we have no mysterious inherited potentialities to fall back upon. The different nature of the materials, and the different combinations of incident physical forces by which the changes are brought about, are the only factors entering into the problem. But the intimate nature of the living matter of which the pellicle is composed, may be understood to vary almost *ad infinitum* according to the nature and quantity (relative and absolute) of the organic ingredients entering into the composition of the infusions, and the degree in which they have been modified by the action of heat. And when we consider how these varying initial combinations may have been acted upon by many different combinations of physical forces, both during the evolution of the primary organic units and also during the secondary or derivative origin of the

comparatively higher forms of life, we can, at all events, see our way to expect that such diverse combinations of matter and motion might result in an almost endless diversity of organic form. But we should expect that given sets of conditions acting upon essentially similar organic materials would give rise, on different occasions, to organisms more or less similar. So that we can only conclude that constantly recurring forms are to be looked upon as the result of certain original combinations of matter and motion, and subsequent interactions, more or less similar in kind, between such initial combinations and their environments. The forms of elementary organisms ought to have a generic constancy and correspondence with the conditions under which they have been evolved, similar to that which we find in the case of crystals—though of course with a much wider range of possible variation. And as slight variations in one or other of the factors may be continually anticipated, rather than absolute similarity, we should be led to expect that organisms so evolved would be liable to present many variations in form—that, though exhibiting characters generally similar, they should nevertheless be found to differ continually in matters of minor detail.

Animals or plants that have been derived from similar parents which multiply sexually, possess an inherited tendency to develop in given directions, and thus the type or pattern is more likely to be perpetuated. But Infusoria and Fungus-germs evolved from a pellicle—

new made, and boasting of no line of ancestors—are the products of the very conditions and matter in and from which they are produced, and are therefore liable to present endless minor modifications on different occasions.

We can confidently appeal to those who have carefully studied the organic forms which teem in infusions, to bear us out in the assertion that they do exhibit all this diversity which, owing to their nature and mode of origin, we have been supposing they ought to present. We have already spoken of the extreme diversity amongst the primary organic forms—such as *Bacteria*, *Torulæ*, and their allies; and with regard to the lowest microscopic Fungi and the lowest microscopic Algæ, their inconstancy of specific form is most proverbial[1]. Similarly, the extreme variability of the Ciliated Infusoria was well known and commonly noted even by the older naturalists, whether they were believers in specific identity or not.

Both Gruithuisen[2] and Tréviranus[3] say that the infusoria met with have never presented similar characters when they have been encountered in different infusions; nor have they been more uniform in the same infusion when different portions of it have been exposed to the incidence of different conditions. The slightest variations in the quantity or quality of the

[1] See *Appendix* E, pp. lxix and lxxvi.

[2] 'Organozoonomie,' p. 164. Munich, 1811.

[3] See Müller's 'Physiology,' translated by Baly, pp. 12 and 13.

materials employed was invariably accompanied by the appearance of different organisms—these being oftentimes strange and peculiar, and unaccompanied by any of the familiar forms. Even Ehrenberg himself was obliged to admit the difficulty of obtaining similar infusoria in apparently similar infusions; and Burdach also records the fact that the characters of these animals vary with the medium, and with the conditions under which they exist. Dujardin, too, one of the more recent systematic writers, acknowledges the great difficulty there is in establishing zoological distinctions between these animals. He refers to the *Paramecia*, more especially, as forms which are prone to undergo the greatest variation under the influence of the smallest change of conditions. M. Pouchet, moreover, says he has continually seen new forms arise in solutions, which never again presented themselves to his observation, even in the course of years. The morphological diversity of these animals is so great that it is always extremely difficult, and often quite impossible, to determine their names. 'Sometimes,' he adds[1], 'a multitude of different forms appear even in a single experiment. Thus in a maceration of some fragments of human bone, which I had brought from the catacombs of Thebes, and which had remained three months in water, I saw the greater number of the Vorticellæ of our French fauna present themselves at once, and in addition a great number of other forms

[1] 'Hétérogénie,' p. 412.

which I think have never been represented. It was quite a new world.' Ehrenberg was convinced that twelve species described by O. F. Müller as belonging to the genus *Vorticella*, were only different modifications of one and the same species. And yet these twelve forms were so different that Lamarck and Bory de Saint-Vincent ranged them under several different genera.

Again, although much uncertainty still prevails as to the extent of modification which the Ciliated Infusoria may undergo, and as to the reality of some of the marvellous metamorphoses that have been alleged to occur, still the almost innumerable variations in their modes of reproduction[1], are quite in accordance with such alleged modifiability. The absence in them of any hereditary tendency to develop in a given direction or after a definite fashion, should render such organisms extremely liable to change throughout their whole life. Having no constant tendency to assume any given form, and possessing an extremely simple organization, there is an absence of the usual conservative influence which opposes the occurrence of internal changes and resists the modifying agency of altered external conditions. The forms, therefore, which such organisms may assume during their growth may be expected to vary in a most marked manner from time to time. And this also is a well-known and thoroughly

[1] See *Appendix* E, pp. xcvii–cv.

recognized characteristic of Infusoria, concerning which we shall have more to say hereafter.

It will, of course, be seen that the phenomena which we have described as taking place in the 'proligerous pellicle' may be watched by all who are conversant with such methods of investigation. We do not require to call in the aid of the chemist, we need exercise no special precautions; the changes in the pellicle are of such a kind that they can be readily appreciated by any skilled microscopist.

Just as we have supposed that living matter itself comes into being by virtue of combinations and rearrangements taking place amongst invisible colloidal molecules, so now does the study of the changes in the 'pellicle' absolutely demonstrate the fact that the visible, new-born units of living matter behave in the manner which we have attributed to the invisible colloidal molecules. The living units combine, they undergo molecular rearrangements, and the result of such a process of Heterogenetic Biocrasis is the appearance of larger and more complex organisms; just as the result of the combination and rearrangement between the colloidal molecules was the appearance of primordial aggregates of living matter. Living matter is formed, therefore, after a process which is essentially similar to the mode by which higher organisms are derived from lower organisms in the pellicle on an organic infusion. All the steps in the latter process can be watched; it is

one of synthesis—a merging of lower individualities into a higher individuality. And although such a process has been previously almost ignored in the world of living matter, it is no less real than when it takes place amongst the simpler elements of not-living matter. In both cases the phenomena are essentially dependent upon the 'properties' or 'inherent tendencies' of the matter which displays them.

CHAPTER XVIII.

THE PANSPERMIC HYPOTHESIS.

Reasons why this subject has been deferred. Has more to do with Heterogenesis than with Archebiosis. Why 'Germs' were supposed to be necessary. L'Emboîtement and Panspermism—Bonnet and Spallanzani. Their views founded on Fancy rather than Fact. M. Pasteur's observations as to presence of Germs in the Air. His so-called 'corpuscles organisés.' His modifications of Panspermic Theory. M. Pouchet's observations as to Nature and abundance of Solid Bodies in the Atmosphere. Work of other Observers. Probabilities of the case. M. Pasteur's Assumptions. Difficulty of explaining Facts in accordance with the Panspermic Doctrine. Mode of origin of Ciliated Infusoria not affected by its Truth or Falsity. Modes of Reproduction of Ciliated Infusoria. Fission. Development of Ova. Comparative Experiments with different Fluids similarly exposed. Occurrence or non-occurrence of Infusoria dependent upon Will of Experimenter. Influenced by thickness of Pellicle. Also influenced by Nature of Infusion. Presence and kinds of Organisms proved to be more dependent upon this than upon Germs in Air or Water. Reasons for referring especially to Origin of Ciliated Infusoria rather than that of Fungi. These proceed from large and easily recognizable Germs. Utterly untenable nature of Panspermic Hypothesis.

WE have already shown that the *de novo* origin of living matter can be established without reference to investigations concerning the number or nature of the germs contained in the atmosphere. With the view of avoiding all chances of error, we

are bound, in such an enquiry, to act on the supposition that germs may be universally distributed through the air, water, and all other substances employed. Accepting such possibilities, and acting as carefully as if they had been established truths, we have nevertheless been able to show that living matter does make its appearance within sealed vessels in which, as we are fully entitled to believe, all pre-existing living matter had been destroyed.

Further, we have pointed out that the microscope alone may teach us how little all that has been previously written concerning the universal distribution of germs has any direct bearing upon the question of the mode of origin of the primordial forms of life[1]. It is useless to look in the air for invisible germs; and yet the careful microscopical examination of films of fluid[2] teaches us that if *Bacteria* arise from pre-existing germs at all, these germs must have pre-existed in an invisible state[3].

In the present stage of our enquiries, however,—now that we are engaged in tracing the organic forms successively assumed by new-born living matter and its derivatives in infusions, or other fluids, exposed to the air—it becomes desirable that we should know what

[1] See vol. i. p. 297; and 'Nature,' 1870, No. 47, p. 410.

[2] See vol. i. p. 295.

[3] It has been shown, not only that *Bacteria*, *Torulæ*, and other forms can arise *de novo*, but also that the air does not contain any appreciable number of *Bacteria* germs, whether visible or invisible. (See pp. 5–7.)

amount of truth there is in the Panspermic hypothesis [1]. Although the startling revelations made to us by the microscope in this field of research are of such a nature as scarcely to admit of any other interpretation than that which we have given, still it is now desirable—partly in regard to these investigations, and partly with reference to subsequent enquiries—that we should know what kinds of living things are to be met with in the atmosphere, and whether or not they are abundantly represented.

In the first sentence of his 'Considerations sur les Corps Organisés,' the celebrated Charles Bonnet explains the reasons which have been instrumental in bringing about all the discussions concerning the existence and distribution of 'Germs.' He says that philosophy being unable, in accordance with known laws, to explain the mode of formation of organized beings, 'happily conjectured that they existed already in miniature, under the form of Germs or Organic Corpuscles,' and then goes on to state that this idea gave rise to two hypotheses [2]. He adds: 'La première suppose que les Germes de tous les Corps organisés d'un même espèce, étoient renfermés, les uns dans les autres, et se sont

[1] It will, of course, now (after all the evidence we have adduced) be held probable that most of the *Bacteria* of infusions are derived from others which have been evolved *de novo*—even in solutions exposed to the air.

[2] Liebig says:—'In the earliest period it was believed that metals were developed from a seed or germ; at a later period the opinion prevailed, that the chemical process generated the seed.' ('Letters on Chemistry,' 3rd ed. p. 69.)

développés successivement[1]. . . . La seconde hypothèse répand ces Germes partout, et suppose qu'ils ne parviennent à se développer lorsqu'ils rencontrent des *Matrices* convenables, ou des Corps de même espèce, disposé à les retenir, à les fomenter, et à les faire croître.' The first hypothesis, which seeks support from considerations regarding the infinite divisibility of matter, is that of Bonnet (De l'Emboîtement), whilst the second (Panspermism) is that with which the name of Spallanzani is more especially associated.

Speaking of his own doctrine, Bonnet says:—'La première hypothèse est un des grands efforts de l'esprit sur le sens. Les différens ordres d'infiniment petits abîmés les uns dans les autres, que cette hypothèse admet, accablent l'imagination sans effrayer la raison[2]. Accoutumée à distinguer ce qui est du ressort de l'entendement, de ce qui n'est que du ressort du sens, la raison envisage avec plaisir la graine d'une plante ou l'œuf d'un animal, comme une petite monde peuplée

[1] Bonnet describes this as a doctrine of Evolution; and this word was commonly employed during the latter part of the last and the early part of this century, in reference to such a process of unfolding of pre-existing germs, and in opposition to Harvey's doctrine of Epigenesis. Although the word 'Evolution' now carries with it quite a different significance in the minds of those who have studied Mr. Herbert Spencer's 'System of Philosophy,' it is still occasionally used in its old sense. (See Prof. Owen's 'Anat. of the Vertebrates,' vol. iii. 1868, p. 809.) The doctrine of Epigenesis, in fact, now forms part of the modern doctrine of Evolution.

[2] At the present day, most people would be disposed to think that the words 'raison' and 'imagination' ought to have changed places with one another in the above sentence.

d'un multitude d'Etres organisés, appellés à se succeder dans toute la durée de siècles. . . . Le Soleil un million de fois plus grand que la Terre a pour extrême un globule de lumière, dont plusieurs milliards entrent à la fois dans l'œil de l'animal vingt-sept millions de fois plus petit qu'un Ciron. . . . . Mais la raison perce encore au delà de ce globule de lumière, elle voit sortir un autre Univers qui a son soleil, ses planètes, ses végétaux, ses animaux, et parmi ces derniers un animalcule qui est à ce nouveau monde ce que celui dont je viens de parler, est au monde que nous habitons[1].' Such a conception as this, although perfectly legitimate as a mere fancy, will appear to most people who calmly reflect upon the subject, utterly without claim or title to influence their judgment. An erroneous or inadequate notion concerning the processes of development which occur in the higher organisms probably induced Bonnet to originate a doctrine which he would not otherwise have countenanced[2].

[1] 'Considérations sur les corps organisées.' Amsterdam, 1772.

[2] On this subject Mr. G. H. Lewes writes with his usual felicity:—'Although we can only by a fallacy maintain the oak to be contained in the acorn, or the animal contained in the ovum, the fallacy is so natural, and, indeed, so difficult to escape, that there is no ground for surprise when physiologists, on first learning something of development, are found maintaining that the perfect organism existed already in the ovum, having all its lineaments in miniature, and only growing into visible dimensions through the successive stages of evolution ("Nulla in corpore animale pars ante aliam facta est, et omnes simul creatæ existunt." Haller, 'Elementa Physiologiæ,' viii. 148.) The preformation of the organism seemed an inevitable deduction from the opinions once universal. It led to many strange and some absurd conclusions; among

According to Spallanzani, on the other hand, all things—earth, air, and water, bodies organic and bodies inorganic—were saturated with 'germs,' or potential living things. This notion was not quite so extravagant as that of Bonnet, though it was equally without legitimate foundation in the actual knowledge of the time[1]. It was put forward to explain certain facts—and the theory itself was then supposed to be substantiated by the occurrence of these same facts. This was the vicious circle by means of which the hypothesis was supported. It was believed in only too readily by the majority, because it enabled them to stave off for a period the acceptance of views which the state of science and philosophy at that time rendered it difficult for them to accept. Spallanzani thought that the air carried with it everywhere the germs of myriads of elementary organisms; or, at all events, some '*principes préorganisés*,' invisible and ideal, which we can only compare with the disembodied spirits whose existence is postulated by the Pythagorean philosophers.

> 'Morte carent animæ: semperque, priore relictâ
> Sede, novis domibus habitant, vivuntque receptæ.'

Spallanzani, nevertheless, was not, on all occasions,

them, to the assertion that the original germ of every species contained within it all the countless individuals which in process of time might issue from it; and this in no metaphysical "potential" guise, but as actual boxed-up existences (*emboîtés*); so that Adam and Eve were in the most literal sense progenitors of the whole human race, and contained their progeny already shaped within them, awaiting the great accoucheur, Time.' ('Fortnightly Review,' June, 1868, p. 593.)

free from honest doubts as to the real nature of such intangible wanderers, and on one of these occasions he frankly said[1]:—'Les infusoires tirent sans doute leur première origine de principes préorganisés; mais ces principes sont ils des œufs, des germes ou d'autres semblable corpuscles? S'il faut offrir des faits pour répondre à cette question, *j'avoue ingénument que nous n'avons sur ce sujet aucune certitude.*' So far then we meet with nothing but the wildest hypothesis[2]. This seems

[1] 'Opuscules de physique animale et végétale.' Pavie, 1787, tom. i. p. 230.

[2] These doctrines of Bonnet and Spallanzani are perhaps mostly due to the influence of the antecedent, though then all-powerful, teachings of Leibnitz. The 'Monads' of this celebrated philosopher may be said to have replaced the 'atoms' of the ancient Greeks. We have no longer, however, to do with a universe composed of corporeal and extended units, we have in their place unextended though dissimilar centres of force, mere metaphysical points; yet, nevertheless, they are supposed to be living things,—even souls—endowed with different degrees of perceptive power. Everything that existed was, according to Leibnitz, replete with life—nay, actually a mass of living individualities: the whole universe was spiritualized. What similarities there were between the conceptions of Bonnet, to which we have referred, and those of Leibnitz (which probably suggested them), may be gathered from the following reference to the doctrines of the great German philosopher:—'As it is with the human soul, which sympathizes with all the varying states of nature, which mirrors the universe, so it is with the monads universally. Each—and they are infinitely numerous—is also a mirror, a centre of the universe, a microcosm: everything that is or happens is reflected in each, but by its own spontaneous power, through which it holds ideally in itself, as if in germ, the totality of things. By him, then, who shall look near enough, all that in the whole huge universe happens, has happened, or will happen, may, in each individual monad, be, as it were, read.'—('Schwegler's History of Philosophy,' translated by Stirling, p. 196.)

to be generally admitted; and, indeed, when speaking of the position of the germ theory anterior to the labours of M. Pasteur, one of its warmest adherents, M. Milne-Edwards, says[1]:—'Jusqu'alors l'existence de propagules ou de germes d'Infusoires dans l'atmosphère était une hypothèse plausible pour expliquer l'origine de ces êtres d'une manière conforme aux lois générales de la reproduction; mais c'était une supposition seulement, et l'on n'avait pu ni voir ni saisir ces corpuscles reproducteurs.'

M. Pasteur was, however, not the first who had endeavoured to obtain experimental evidence as to the truth of the panspermic hypothesis. As M. Pouchet points out, he had been preceded by M. Baudrimont and by M. Gigot, if not by others. The former shook up large quantities of air with small quantities of water, and afterwards submitted the water to microscopical examination without finding any recognizable eggs or spores[2]. M. Gigot[3], on the other hand, made use of an aspirator in order to draw the air of marshy districts through dilute sulphuric acid, and by this means he filtered out a certain amount of organic *débris*.

M. Pasteur has, however, endeavoured most assiduously to take the 'panspermic' doctrine out of the re-

[1] 'Anat. et Physiol. Comp.' t. viii. p. 264.

[2] See 'Observations des êtres Microscopiques de l'Atmosphère Terrestre,' *Compt. Rend.* 1855, t. xli. p. 542. His observations did not accord, therefore, with the recent marvellous statements of Mr. Dancer.

[3] 'Recherches Experimentales sur la Nature des Emanations Marécageuses.' Paris, 1859.

gions of mere hypothesis. He has striven to establish its truth by observation and experiment, and has given full details as to the methods which he adopted in his 'Mémoire sur les Corpuscles Organisés qui existent dans l'Atmosphère[1]'. At first he entirely adopted the views of Spallanzani, that germs existed everywhere in the atmosphere and were universally diffused, though he afterwards maintained a modified form of this doctrine. The results of his experiments forced him to come to the conclusion that certain parts of the atmosphere contain no germs. He is now, therefore, compelled to surmise that they probably exist in veins or areas, variously interblended with germless portions of the atmosphere. He thus expresses himself:—'In conclusion, we see that ordinary air contains only here and there, and with no continuity, the necessary condition for the initiation of the so-called spontaneous generation. Here there are germs, whilst in immediately adjoining portions of the atmosphere there are none. Further on there are other kinds of germs, and there are few or many of them according to the nature of the locality.' In addition to what were supposed to be organized corpuscles, other fragments and foreign particles of the most varied nature were met with—though the kinds, relative proportions, and actual abundance of the different solid bodies, varied extremely with the nature of the locality in which the air was examined and also with the state of the atmosphere

[1] 'Ann. de Chimie et de Physique,' tom. lxiv. 1862.

at the time. In air obtained from the Rue d'Ulm, in Paris, M. Pasteur encountered, in addition to the particles which he supposed to be of an organized nature, a large quantity and a great variety of foreign ingredients.

In these researches M. Pasteur made use, as M. Gigot had done, of an aspirator, by which air was drawn at a definite rate (by a process of filtration) through a glass tube containing a pledget of gun-cotton sufficiently large to arrest all solid particles amongst its fibres. After a known quantity of air had been filtered in this way, the gun-cotton was removed and dissolved in a mixture of æther and alcohol. This was allowed to stand, so that all the solid particles which had been entangled amongst its meshes, having been liberated, might gradually sink to the bottom of a conical glass. They were afterwards washed several times with distilled water; and an interval of twelve hours was left between each washing, so that all the corpuscles might have time to subside before the supernatant fluid was withdrawn. After five or six washings the residuum was poured out into a watch-glass, whence any excess of fluid soon evaporated. The particles thus obtained could easily be placed upon an ordinary slide, and examined under the microscope with the aid of various reagents. These examinations convinced M. Pasteur that ordinary air contains a considerable though still a variable number of corpuscles, whose form and structure made him think they were organized. The corpuscles varied from the smallest appreciable size up to

$\frac{1}{2500}''$ in diameter, or they might be even larger. They were either spherical or ovoid, with more or less sharply-defined borders. Some were quite translucent, whilst others were more opaque, owing to the presence of actual granules, or, at all events, to a granulated appearance in their interior. Those which were translucent and sharply defined had the closest resemblance, M. Pasteur says, to the spores of fungi; whilst amongst the other materials, bodies resembling encysted Infusoria were occasionally found, and also globules resembling the eggs of these creatures. 'Mais quant à affirmer,' he says, 'que ceci est un spore, bien plus la spore de telle espèce déterminée, et que cela est un œuf et l'œuf de tel microzoaire, je crois que cela n'est pas possible.' M. Pasteur could, in fact, make no more definite statement concerning them—he could only announce his own impression that they were organized bodies of some kind[1].

We must, therefore, bear in mind that even these experiments of Pasteur have only sufficed to bring to light certain minute particles, having a general resemblance to spores of fungi or ova of Infusoria. He found nothing which he could state was the product of such or such organism, or which he has absolutely proved to be organized, by having watched its develop-

[1] Referring to the figures of these bodies given in M. Pasteur's Memoir, Prof. Owen says ('Anat. of Vert.' vol. iii. 1868, p. 814):—'Of the various well-marked forms of ova or germs of lower organisms, I know not any recognizable in the figures above cited.' See also M. Robin's 'Traité du Microscope,' 1871, p. 821.

ment under the microscope, into any fungus or variety of Infusorial animalcule. Obviously, this is the only kind of evidence which is strictly admissible—and yet such evidence M. Pasteur has not attempted to adduce[1].

The evidence by which he has attempted to prove that the 'corpuscles' are really germs, is of by no means so satisfactory a nature, or so free from chances of misconception as could be desired[2]. Bearing in mind, therefore, that the so-called 'germs' of Pasteur have not been proved to be such, we feel bound to say that experiments have done very little indeed, even in the hands of so skilled an operator, to raise the panspermic doctrine out of the region of mere hypothesis. We have seen, in fact, that M. Pasteur himself now holds the doctrine only in an extremely modified form. And yet his researches are constantly referred to in such terms as to lead others to believe that they had definitely established the truth of the 'panspermic' hypothesis.

M. Pouchet has also examined the air of the most varied localities with the greatest care by several dif-

[1] In a note appended (p. 34) to this part of his Memoir, M. Pasteur says, 'Ce qu'il y aurait de mieux à faire et de plus direct consisterait à suivre au microscope le développement de ces germes. Tel était mon projet; mais l'appareil que j'avais fait construire pour cet objet ne m'ayant pas été livré en temps opportun, j'ai été éloigné de cette étude par d'autres travaux.'

[2] Nevertheless, M. Pasteur does actually claim to have *proved* the correctness of his supposition. (See p. 17.)

ferent methods. At first he made use of a simple aspirator, though he afterwards worked with the aid of an *aëroscope*—an instrument which he had himself devised[1], and by means of which he concentrated upon a space of glass, two millimetres square and moistened with glycerine, all the corpuscles and foreign particles disseminated through a cubic metre or more of the atmosphere[2]. Such examinations soon convinced him that the air of different localities varies very much as to the nature of the particles which may be obtained therefrom, but they also convinced him just as much of the extreme rarity with which real spores or ova are to be encountered.

He says:—'I submit to the aëroscope the atmosphere of towns and of marshes, that over the sea, and that in mountain regions. In the first-named localities I find it always surcharged with an infinite variety of organic débris, and of that from other substances made use of in our daily life. In that of the marshes and plains one meets with an enormous quantity of fragments of vegetable tissue. On the contrary, over the sea far from shore, and on the mountains above the zone of human habitation and of vegetable life, corpuscles of any kind in the atmosphere become infinitely rare and infinitely

[1] Described in 'Compt. Rend.' t. l. p. 748.

[2] Dr. Maddox has also recently described a somewhat similar 'Apparatus for collecting Atmospheric Particles,' which seems to be very portable and well adapted for the purpose. (See 'Monthly Microscopic Journal,' June, 1870.)

small even in a volume of air equal to ten cubic centimetres—which, in reference to such experiments, must be considered something considerable. In such a volume of air I have not yet met with anything which could be supposed to be either starch-granules, eggs of Infusoria, or spores of Mucedineæ[1].'

It also occurred to M. Pouchet that the nature of the solid materials floating in the air of different localities might be tested by examining the respiratory passages of different animals—and more especially those of birds, in which intercommunicating air-sacs ramify extensively throughout the osseous system. He examined, therefore, with the greatest care those bones of birds which were most pervious to air; since corpuscles or fragments of any kind when once they had been introduced into such cavities would be likely to lodge, on account of the great irregularities of their surface[2]. He says:—'When studied in this manner, the respiratory apparatus gives us a faithful idea as to the life of these animals. It not

[1] 'Compt. Rend.' tom. li. p. 534. And yet, making use of a single cubic decimetre of this very air taken either on the sea between Sardinia and Sicily, or from the top of Mount Etna, M. Pouchet always obtained immense legions of Infusoria in his solutions after a very short time.

[2] M. Pouchet says ('Nouvelles Expériences,' p. 79):—'Pour recueillir les corpuscles aériens des os pneumatiques des oiseaux, j'enfonce le tube d'une seringue dans l'ouverture par laquelle l'air pénètre dans leur cavité, et je coupe l'os vers l'extrémité opposée. L'eau injectée d'abord doucement puis ensuite très-violemment pour entraîner jusqu'aux moindres débris atmosphèrique est reçue dans des verres et examinée.' Every precaution was also taken to avoid admixture of particles from the atmosphere at the time of examination.

only reveals what habitats they prefer, and the nature of their food, but even, when they are domestic, the trade or occupation of those amongst whom they have lived.' He found that the long bones of birds living in towns and in the neighbourhood of human habitations contained an abundance of particles of carbon, and filaments of the various kinds of textile fabrics, in addition to different sorts of starch particles[1]. But the more the animal lived in regions remote from human habitations, the more rare did such material become—so that in those continually dwelling in the midst of forests nothing of this kind was to be met with. The respiratory cavities of such birds, on the contrary, contained an abundance of vegetable débris—of epidermic tissue and particles of chlorophyll. 'But,' M. Pouchet says[2], 'in all our observations, which without exaggeration one might reckon by hundreds, we have never encountered either a single spore, a single egg of an Infusorial animalcule, or a single one of these in the encysted condition.' 'And,' he adds, 'if in all these minute researches we have succeeded in finding starch everywhere, wherever it existed, is it possible that the spores and the atmospheric eggs could alone have escaped us?' If starch particles could penetrate into such cavities, ova or spores nearly similar in

[1] Valuable additional information on this part of the subject has been furnished by Dr. Sigerson ('Monthly Microscop. Journal,' Aug. 1870).

[2] 'Compt. Rend.' tom. l. p. 1127.

size and specific gravity ought also to be able to penetrate, and to be as easily recognizable.

The wide distribution of starch throughout the atmosphere and in the respiratory organs of animals was first pointed out by M. Pouchet. It presents itself under two principal modifications: (1) in its natural condition, and (2) as it exists after having undergone a process of cooking. In the majority of cases it exists in the former state; but it is also often met with, in the most varied situations, either simply swollen or quite burst by the action of heat[1]. Such swollen or burst granules which are swept about in the atmosphere are probably derived from microscopical fragments of bread. In addition to its swollen or cracked appearance, this form of starch is characterized by the fact that it is not so strongly coloured by iodine as that which is in its normal condition. The particles of starch are met with of almost all sizes below $\frac{1}{750}''$ of an inch in diameter—which is about the magnitude of the largest granules. The larger ciliated Infusoria, however, vary from $\frac{1}{500}''$ to $\frac{1}{250}''$ in diameter; whilst their eggs, according to Balbiani, are not less than from $\frac{1}{1050}''$ to $\frac{1}{1250}''$ in diameter, so that they would be as easily appreciable wherever they existed as the starch granules themselves, which seem to be so much more ubiquitous. The

[1] M. Pouchet also occasionally found starch granules of a bright blue colour. The cause and nature of this colour-modification is very obscure (see 'Compt. Rend.' 1860, t. l. p. 572), though it may be due to the presence of minute quantities of iodine in the atmosphere.

spores of fungi are much smaller, but very many of them are quite large enough to be appreciable wherever they really occur. According to Pineau, those of *Penicillium glaucum* vary from $\frac{1}{9000}''$ to $\frac{1}{8000}''$ in diameter, and M. Pasteur himself gives the dimensions of the spores of the most common species of the genus *Ascophora* as from $\frac{1}{4100}''$ to $\frac{1}{3300}''$ in diameter. All such spores or ova should therefore be almost as easily recognizable as particles of starch. But whilst the latter are encountered with the greatest frequency, the former are only very rarely found.

In the same year, it occurred to M. Pouchet and also, quite independently, to MM. Joly and Musset[1], that the examination of snow flakes would be a very good means of obtaining some knowledge as to the nature of the particles existing in the atmosphere. Their anticipations were fully verified. The large snow flakes did entangle the atmospheric particles, so that—especially during the commencement of a snow storm—they were found to contain a very large quantity of almost all the ordinary varieties of atmospheric particles and fragments: and these differed in nature according to the localities in which the snow fell. But, in addition, M. Pouchet says[2]:—'This snow contained a considerable number of *Protococcus pluvialis*, of a beautiful green colour.' Rain, collected in a suitable vessel as it fell, has also been examined by Mr. James Samuelson, who was one of the earliest of those who paid attention

[1] 'Compt. Rend.' 1860, t. l. p. 647. [2] 'Nouv. Expér.' p. 76.

to the subject in this country[1]. He has lately made more decisive observations by an examination of the rain collected in one of the low, unhealthy parts of Liverpool, and also at Everton on the outskirts of the city. He says[2]:—'On examining the rain which had fallen in both these localities, I found, naturally enough, no animal or plant germs in that from the lower part of the town, although it was highly charged with soot and various kinds of dirt. But in that which had been collected near my house, I found on the same day a few of the unicellular organisms as before, some single, others undergoing subdivisions; also a little soot and silex.' Subsequently the corpuscles were seen to develop and give rise to a fungus mycelium. Then, again, Dr. Braxton Hicks states[3] that he has frequently found bodies resembling the gonidia of Lichens in snow and rain; whilst Ehrenberg has described many forms of Diatoms which have from time to time descended in atmospheric showers[4].

[1] See 'Compt. Rend.' 1863, t. lvii. p. 87.

[2] 'Quart. Jrnl. of Sc.' Oct. 1870, p. 496.

[3] *Appendix* D, p. liv. note 3.

[4] See his 'Passatstaub und Blutregen.' Speaking of 'red snow,' Burdach (t.i.p. 37) cites the following opinions as to its nature and mode of origin:—'La neige rouge qu'on a quelquefois trouvée dans les regions arctiques et sur de hautes montagnes, est suivant Agardh, le *Protococcus kermesinus*, Algue, du plus bas degré, qui se compose de vésicules pleines d'une substance mucilagineuse et grenue, et contenant de la résine, avec d'autres matières végétales. Cette Algue adhère aux pierres ou à la neige, de sorte que le vent ne peut point l'entrainer. Agardh pense que le *Protococcus* est engendré par l'action de la lumière solaire sur la neige fondante ("Nov. Act. Nat. Cur." t. xii. p. 746). Mais les observations de

It seems undoubtedly true, therefore, that living organisms do descend in moderate numbers from unknown heights, with rain and snow; though it seems just as obvious that the air of ordinary localities, near the surface of the earth, only contains a very limited number of recognizable spores or germs of living things. On this subject we have, in addition, the valuable testimony of Prof. Jeffries Wyman, who says[1]:—'We have carefully examined the dust deposited in attics, also that floating in the air, collected on plates of glass covered with glycerine, and have found in such dust, in addition to the débris of animal and vegetable tissues, which last were by far in the greatest abundance, the spores of Cryptogams, some closely resembling those of confervoid plants; and with them, but much less frequently, what appeared to be the eggs of some of the invertebrate animals, though we were unable to identify them with those of any particular species. We have also found grains of starch in both kinds of dust examined, to the presence of which

Nees sur la grêle rouge et sur une espèce de pline rouge (loc cit, t. i. p. 573) rendent plus probable que, comme l'admettait aussi Wrangel (Ibid. p. 351), cette Algue se forme dans l'atmosphère, que c'est pas conséquent un aérophyte et qu'elle se produit, dans les temps d'orages et de météores ignés.' And elsewhere (p. 24) he says:—'Zimmermann ("Archiv. für die gesam. Naturlehre," t. i. p. 257) a trouvé dans de l'eau météorique une substance organique particulière différente du mucus et de l'extractif, et qui dégageait de l'ammoniaque en se décomposant; cette substance, appelée par lui *pyrrhine*, y était mêlée avec du fer, du manganèse, de la chaux, de la magnésie, et de l'acide hydrochlorique.'

[1] 'American Journal of Science,' July, 1862.

Pouchet was the first to call attention. When compared with the whole quantity of dust examined, or even with the whole quantity of organic matter, both eggs and spores may be said to be of rare occurrence. We have not in any instance detected dried animalcules, which were resuscitated by moisture; and when the dust has been macerated by water, none have appeared until several days afterwards, until after a lapse of time when they would ordinarily appear in any organic solution.' The testimony of Dr. Maddox is also very much to the same effect—as to the comparative paucity of recognizable living germs, even when large quantities of air are made to deposit their floating contents on a limited area of glass, covered by a viscous material'[1].

It will be perceived, therefore, that in all these attempts to ascertain the nature of the solid particles in the atmosphere, even large quantities of air constantly renewed serve only to yield evidence of the most sparing distribution of spores or ova. But, if we limit ourselves to the employment of means which are more exactly comparable with the atmospheric conditions to which our infusions are exposed, we discover a far greater paucity of reproductive particles. If we place

[1] 'Monthly Microsc. Journal,' June, 1870, p. 290. Other contributions have been made to our knowledge of this subject which are too numerous to mention. We may, however, cite valuable papers by Dr. Sigerson and Dr. Arthur Ransome, which are to be found in the same volume of the 'Monthly Microsc. Journal.'

beneath the same bell-jar a narrow vessel containing an organic infusion, and on a level with the surface of this, a plate of glass smeared with glycerine covering an equal area, we have two surfaces which are equally exposed to the access of atmospheric germs[1]. Yet, in the course of a few days, the organic infusion will swarm with infusoria, whilst the film of glycerine will, in the majority of cases, show nothing more than a very minute quantity of organic and inorganic débris, mixed with a few particles or 'corpuscles,' which, when organic, seem for the most part to be nothing more than mere dead particles, resulting from the disintegration of organic matter.

The briefest reflection upon the probabilities of the case seems to suggest that this is most likely to be the nature of the majority of 'particles' and 'globules' which are encountered by all observers in this kind of

[1] If atmospheric germs are to fertilize the organic infusion. they must be brought into contact with its surface either by gravitation or movement of the air. The surface of glycerine would therefore be the very best index as to the amount and nature of the particles which drop into the infusion from the atmosphere. The inorganic particles and the heavier organic particles have the greatest tendency to subside. Dust which has been deposited from the atmosphere has been ascertained by Dr. Percy and also by Mr. Tichborne ('Chemical News,' Oct. 1870) to contain only from one-half to one-third per cent. of organic matter, though amongst the finer particles, which remain longer suspended in still air, the proportion of organic matter is probably much larger. We are all familiar with the specks and motes which dance in the sunbeam, but Prof. Tyndall has conclusively shown ('Nature,' No. 13, 1870) that the electric light is a far more potent means of revealing the presence of otherwise invisible impurities both in air and in water.

research. The surface of the earth is clothed with living things of all kinds, animal and vegetal, which are not only continually throwing off organic particles and fragments during their life[1], but are constantly undergoing processes of decay and molecular disintegration after their death. The actual reproductive elements of these living things are extremely small in bulk when compared with other parts which are not reproductive. When, moreover, it is considered that in the neighbourhood of populous cities (the air of which alone exhibits this very large quantity of impalpable, mixed with palpable, organic dust), there is constantly going on a wear and tear of the textile fabrics and of the organic products of various kinds subservient to the wants of man; and that the chimneys of manufactories and dwelling-houses are continually emitting clouds of imperfectly consumed organic particles, some idea may be gained of the manifold sources whence the organic particles and fragments found in the atmosphere may emanate, and also as to what proportion of them is likely to be composed of living or dead reproductive elements.

[1] Epithelial cells and the débris of such bodies are generally obtainable from the air of ill-ventilated dwelling-rooms, when it is passed through the aëroscope. These off-cast units, as well as pus corpuscles, become much more abundant in hospital wards—especially when they are overcrowded and contain patients with open wounds. The presence of such off-cast elements and particles in the atmosphere is one important means by which the spread of contagious diseases amongst men, and also amongst the lower animals, is brought about. (See *Appendix* E, p. cxliv.)

What, then, are we to say to the assumptions of M. Pasteur, that certain rounded structureless particles are indeed 'organized corpuscles' or the 'germs' about which the panspermatists talk so much? To say nothing of the important modifications which M. Pasteur has felt himself compelled to make in reference to the old doctrines of Spallanzani [1], we find that all which he has yet been able to establish as a matter of certainty, and as a result of actual observation, is, that the atmosphere of certain regions does contain a very appreciable quantity of extremely minute more or less spheroidal and transparent particles, which he (M. Pasteur) assumes to be the much-talked-of germs. But no direct proof of this has ever been adduced. Let us not deceive ourselves, either, as to the amount of similarity between the germs in question and the particles actually found—it is of a negative rather than of a positive description. M. Pasteur is not able to say that the spore of a fungus has such and such structural peculiarities, and that the bodies which he has found present similar definite characters. Such evidence would be cogent in direct proportion to the number and variety of details of structure in respect of which the two bodies were found to correspond. Here, however, the case is quite different, and the value to be set upon the similarity presented undergoes a corresponding decrease. The spores of some of the microscopic fungi, as M. Pasteur says, are mere little spheri-

[1] See pp. 272 and 274.

cal and translucent particles of a tolerably definite size; and then, amongst the materials filtered from the atmo-

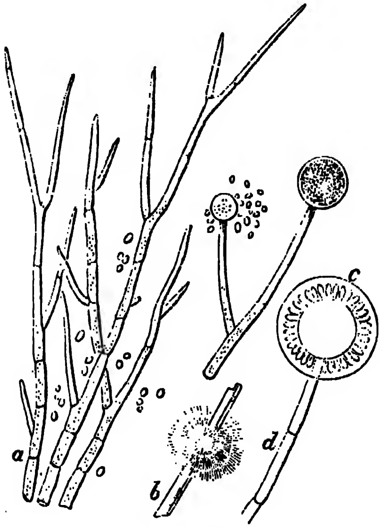


FIG. 66.

Fungus with minute Spores, found in a Closed Flask. (Pouchet.)

a. Mycelial filaments, magnified. *b*. Unmagnified tuft.
c. Spore-case, more highly magnified.

sphere similar little spherical particles exist, also devoid of specific characters: *ergo*, it is argued, these are the very bodies required, these *are* in fact organized germs! So far, the observations and reasonings of M. Pasteur have no other cogency than this, however much he may seem to have verified his assumption by other experiments. The danger of mistaking similarity for identity, when dealing with such general

characteristics, can never be kept too prominently in view—especially when the promulgation of all-important doctrines is to hinge upon our decision.

It should be clearly understood, however, that the question as to the nature of the particles contained in the atmosphere is quite independent of the other enquiry, whether ciliated Infusoria or Fungi can be evolved in organic solutions without ordinary parentage. *Bacteria* and some Fungi have been proved by the only kind of evidence which can ever be obtained, to arise *de novo*; and whilst this proof is possible even by acting on the assumption that the Panspermic hypothesis is true, many of the doctrines of Heterogeny may also be established quite irrespectively of the truth or falsity of the atmospheric germ-theory. It has never been maintained that specimens of the genera *Paramecium* and *Kolpoda* are capable of being directly evolved out of a putrescible organic solution. The necessary and invariable preliminary is that innumerable *Bacteria* should be produced in the infusion, which, by their subsequent aggregation, may form a material out of which the much higher ciliated Infusorial animalcules may be evolved by slow and definite stages, capable of being watched by all skilled microscopists. It would seem, then, even worse than childish to be looking about in the air for germs of these animals. Why did not those who doubted look rather more diligently through their microscopes, to ascertain whether or not

such phenomena would take place, as Pineau, Pouchet, and others declared they had seen? If it did not occur, and others could be convinced of the truth of this, then one of the strong points in support of the doctrines of the heterogenists would have been at once swept away. And even if ova of these Infusoria had been found in association with other matters filtered from the atmosphere, we do not see how it could have seriously affected the doctrines of the heterogenists, so long as their statements concerning the mode of evolution of these animals was capable of being verified. It would soon have appeared probable to most who were capable of forming a judgment upon the question, that the teeming multitudes of ciliated Infusoria, which so rapidly appear in organic solutions, were more likely to have originated, in great part, after this established mode of development, than to have been the offspring—either by means of buds or fission—of two or three solitary animalcules which may have dropped into the solution in a dried condition; or of two or three ova that had accidentally obtained access to the infusion [1], and which, after developing into organisms, may also have multiplied by budding or fission.

It would, undoubtedly, be altogether inconsistent with known facts if we were to assume that such teeming myriads of ciliated Infusoria as are frequently

[1] All the known ova or embryos of these ciliated Infusoria are much too large to pass through the pores of ordinary blotting paper. So that filtering the fluid ensures its freedom from such organisms.

met with in infusions after five or six days, could have been derived from the multiplication of a few solitary individuals, even by the combined methods of fission, budding, and the so-called sexual reproduction.

It is often stated that Ciliated Infusoria multiply very rapidly by means of fission. But even towards the close of the last century Gleichen[1] declared that during the fifteen years in which he had been continually watching these animals, he had only observed a process of fission occur three times; and it was only after some years of observation that De Blainville[2] was actually able to satisfy himself that such a mode of division might take place. He subsequently saw it occur occasionally in certain specimens belonging to the genus *Kolpoda*. Of late years, also, similar testimony has been given on this subject. Mantegazza declares that he has only seen ciliated Infusoria undergo such a process of division two or three times, though millions of these animals of different species had passed under his observation during a space of fourteen months; while M. Pouchet, during observations extending over many years, says he has never once seen a *Paramecium* divide. Specimens of *Kolpoda* he has however more frequently found presenting appearances suggestive of fission. But with regard to the *Vorticellæ*, which, since the times of Spallanzani, have been described as particularly prone to undergo such a division,

[1] 'Dissert. sur la Génération, &c.'
[2] 'Dict. des Sc. Nat.' tom. lx. p. 144.

Pouchet says that of all the myriads he has seen he has never been able to observe an actual division take place, and only four or five times has he found two individuals so united as to suggest that such a process was taking or had taken place. And although M. Balbiani[1] has, of late, re-asserted the great frequency of the occurrence of fissiparous division amongst *Paramecia*, still, in the face of so many statements to the contrary, it would be well that his observations should be confirmed by other observers. Quite recently, in an interesting paper[2] 'On the Anatomy of Stentor,' Dr. Moxon says he has watched the process of fission, as it occurs in *Stentor cæruleus*. It does not take place longitudinally, but rather in an oblique direction, and Dr. Moxon tells us he has never known the whole process occupy less than four or five hours. In a letter, which he kindly wrote in reply to some of my queries, he says he has also several times watched the process of division in specimens belonging to the genus *Stylonychia*. With regard to these individuals Dr. Moxon says:—'As to the time occupied in the process it was too long to allow of my watching it through. I tried to do so, but found that in two hours very little progress had

[1] 'Compt. Rend.' tom. l. p. 1191. Although the evidence brought forward by M. Balbiani is very strong, it is by no means of such a nature as to make it free from doubt. The rapid multiplication did not take place under the eyes of the observer. And more than that, during the first three or four days, the increase in the number of Infusoria was often very slow.

[2] 'Journ. of Anat. and Physiol.' vol. iii. p. 279. ed. 1869.

been made, so that I was obliged to choose specimens that were nearly divided, in order to observe the final process of separation; and then I had to watch the individual for from one and a half to two hours—which I assure you was rather trying and tedious, as the active little beings were trotting about continually, and I had to preserve them in a rather large live-box, since I found if they were confined too closely the division would not occur. In *Vorticella* and *Epistylis* I have seen the division in progress, but the process was so slow that I never saw it through, and several times when I had watched long they disappointed me, by passing into the "encysted" state instead of completing the division. This may have been partly due to their not having had fresh water enough to suit their health.' Dr. Moxon adds: 'I should say that, as far as I have seen, the larger and more perfect Infusoria do not increase very rapidly in numbers,'—that is, by the acknowledged methods of reproduction, fission, gemmation, and internal production of embryos. My own experience is very similar to that of Dr. Moxon. I have seen the process taking place in various ciliated Infusoria, though by no means frequently, and when it does occur, it has generally been very slowly brought about[1]. M. Haime, moreover, in speaking of the pro-

[1] I have seen it much more frequently, however, in the smaller flagellated Infusoria (*Monads*). With them the fission may be either longitudinal or transverse in its direction, and I have found the process occupy at least fifteen or twenty minutes.

cess of fission in Oxytricha, says[1] that 'some hours' are required for its completion in large individuals.

Though the development of 'ova' in the Infusoria by a sexual process of generation seems now to be generally accepted, on the faith of the recent researches of Balbiani and those of MM. Claparède and Lachmann, no appeal can be made to such a process of reproduction in order to account for the rapid appearance of multitudes of Infusoria in organic solutions. Even those observers whose labours have tended to establish the reality of the process, do not pretend that it is an ordinary mode of multiplication. On the contrary, they maintain that it is an extraordinary method of increase to which the animal resorts occasionally under the pressure of adverse circumstances[2]. The rate of increase, also, could only be extremely slow by this method, since the animals remain in contact for five or six days, and it is not till a further lapse of two or three days that the 'ova' obviously begin to make their appearance[3]. Balbiani's observations were principally

[1] 'Ann. des Sc. Nat.' 1853, p. 122.

[2] When speaking of the process of fissiparous division, M. Balbiani says (loc. cit. p. 1194):—'Nous avons effectivement constaté que ce mode de propagation avait des limites et se terminait invariablement de l'une des trois manières suivantes: ou part la mort naturelle et presque simultanée de tous les individus appartenant à une même cycle, ou par le retour de la génération sexuelle indiquant la fermeture d'un de ces cycles et le commencement d'un cycle nouveau, ou enfin par le phénomène de l'enkystement.'

[3] 'Journal de Physiologie,' 1858, tom. i. p. 346. The process is doubtful in nature; and from the absence of all sexual organs in the two

conducted upon specimens of *Paramecium bursaria*, in each of which there were produced by this process five or six large 'ova' measuring about $\frac{1}{1250}''$ in diameter. These germs underwent the first stages of development and were converted into rudimentary embryos before they made

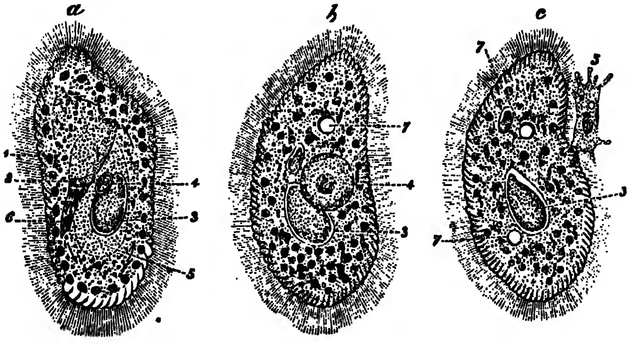


FIG. 67.

Development of Embryos in *Paramecium*. (Cohn.)

their exit from the body of the parent[1]. M. Pouchet has never observed germs within the bodies of *Paramecia*,

similar individuals, it would seem to be most allied to a process of 'conjugation.' It seems better to style the products germs than ova.

[1] The observations of Stein and of F. Cohn had already gone to show that these embryos quit the body of the mother under the form of *Acinetæ*, furnished with clavated tentacles—true suckers by which for a time they remained in contact with the mother, nourishing themselves from her structure. Their observations cease here; but Balbiani has satisfied himself that the embryos soon lose these appendages, which are replaced by cilia. They soon acquire a mouth, by the development of a longitudinal furrow, and thus gradually take on the form of the parent, whilst they develop within themselves the characteristic green granules.

though he has occasionally seen large bodies of this kind existing singly within representatives of the genus *Kolpoda*, and also in specimens of *Kerona*. He says[1]:— 'I have observed this in Kolpodas, which, judging from their great size, appeared to have arrived at their last stages of life.' He states, moreover, that two or three times he saw such 'Kolpodas with their bodies half opened, though still having the egg in the midst of the disorganized structure.' In animals about $\frac{1}{300}''$ in length the germ varied in size from $\frac{1}{1050}''$ to $\frac{1}{1000}''$.

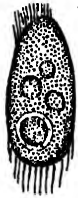


Fig. 68.

Development of Embryos in dying *Kerona*. (Pouchet.)

It was altogether an unmistakeable sort of body, situated near the middle of the animal, and made up of a dense aggregation of fine granules bounded by a transparent vitelline membrane or *zona pellucida*. It was also entirely free within the substance of the organism—in which no trace of an ovarium was to be discovered. In exceptional cases M. Pouchet has seen two other smaller though otherwise similar bodies, adjoining the more fully developed ovum. He has never, however, seen more than three within any single animal, and has

[1] 'Hétérogénie,' p. 400.

never seen a preliminary coupling of two individuals. In specimens of the genus *Kerona* he has occasionally observed this coupling, though he has never seen more than a single germ. The nature of the body in these animals was rendered even more indubitable by the fact that it became converted into an embryo whilst still within the posterior part of the body of the parent. His observations were principally conducted upon specimens of *Kerona lepus* measuring $\frac{1}{140}''$ in diameter, and in which the germ (here again free within the body) was about $\frac{1}{1100}''$ in diameter. M. Pouchet saw a gyration of the embryo, and the characteristic contractile vesicle make its appearance, so as to leave no doubt that the process of development was still advancing[1].

Multiplication by the ordinary processes of repro-

[1] This production of embryos in the substance of dying Infusoria is a subject of much interest. To me it was a matter of special interest to read (in 1869) M. Pouchet's description after I had already, as a result of frequent careful observation, come to the conclusion that the nucleus of the white blood corpuscle was also evolved during the later stages of its life within its very substance; and that it was destined to come to maturity and perhaps, under certain circumstances, take the form of a distinct anatomical element, whilst the rest of the parent structure was about to undergo a process of disintegration (see vol. i. p. 227). This process seems to be most comparable with that by which the embryo is evolved within the body of the Infusorial animalcule. Here also the germ (nucleus) is evolved out of the substance of the parent organism itself, at a time when its own vitality is about to cease. As a sort of link connecting these two sets of phenomena, we may perhaps refer to the development of the moving filaments known as Spermatozoa from the nucleus of the sperm cell. The old element dies in giving birth to the new product; and the new element in this case is an actively-moving, independent zooid.

duction, therefore, will not adequately account for the thousands of ciliated Infusoria which are often to be met with in the course of a few days in many organic infusions; and moreover, as long as we are able to demonstrate that Fungus-spores, Monads, Amœbæ, and Ciliated Infusoria are constantly produced by changes taking place in a pellicle, or living stratum formed by aggregations of *Bacteria*, it is perfectly immaterial whether the air does or does not contain any of these higher organisms or their germs.

Again, in the face of what we know concerning the paucity with which ciliated Infusoria and the microscopic Fungi of infusions are represented in the atmosphere, concerning the extreme rarity of the sexual method of reproduction amongst Infusoria, and also as to the comparative infrequency with which multiplication by fission can be observed, the results afforded by comparisons of cases in which there has been free exposure either of different solutions to the same air, or of portions of the same organic solution under different conditions, are also strongly opposed to the notion of the derivation of such organisms from pre-existing atmospheric germs. The evidence thus obtainable, however, tallies remarkably with the notions of heterogenists as to the principal mode of production of these organisms being from the very substance of the pellicle itself.

If the higher organisms met with after a time in

filtered infusions exposed to the air beneath a bell-jar had been derived from ova or from dried adult forms, which (after dropping into the infusion) had subsequently propagated themselves and multiplied therein, then such ova or dried adult forms ought to drop just as freely into receptacles of distilled water presenting an equal area and similarly exposed. These ova or dried animals are sufficiently large to be easily discoverable, when present; and by placing the water after a time in a conical vessel, any particles which it contains may be allowed to sink before the supernatant fluid is slowly drawn off, either by a pipette, or, better still, by a siphon of small bore. The microscopical examination of the small quantity of fluid which remains will very rarely show a trace of a ciliated Infusorium, either adult or in the form of egg. And yet, if the weather has been warm, in the course of four or five days the surface of the organic infusion similarly exposed beneath a bell-jar will have become covered with a thick pellicle, and the infusion itself, if not affording an acid reaction, may be found to contain an incalculable multitude of ciliated Infusoria, of one form or another.

It may well be asked, whence come these swarming myriads of animalcules? Can they have been derived from certain germs floating in the limited atmosphere to which the infusion has been exposed? If so, one would think that it must be from a very limited number, seeing that none are to be found in the

similarly exposed vessel of distilled water. But on the other hand, the state of our knowledge concerning the rapidity with which these animals multiply by fission and by the still more exceptional sexual method of reproduction is, in turn, equally opposed to such a notion.

Moreover, even if we had not the above-mentioned evidence of the test vessel, the investigations of M. Pasteur as well as those of M. Pouchet would decidedly lead us to reject the notion that any small portion of air contains a plurality of dried Infusoria or of their germs. The refractive granules and supposed germs ('corpuscles organisés') are of much smaller size, and are presumed by M. Pasteur himself to resemble the spores of fungi rather than the much larger and much more definitely constituted germs or dried bodies of Infusoria. He does not pretend to say that these are abundant in the atmosphere, or that they are to be met with in any appreciable number in a limited volume of ordinary air.

Again, the evidence above-cited and that which the microscopist can supply is supplemented by the fact that the pellicle must always be of an appreciable thickness in order that ciliated Infusoria may be produced. Unless this is the case, such organisms are not to be found in the infusion after the accustomed time; or perhaps they may never occur at all, however long a period has elapsed. This has been clearly enough ascertained by the very simple but ingenious experi-

ments of M. Pouchet. He divides into two equal portions a filtered organic solution, favourable for the appearance of ciliated Infusoria, placing the one portion in a tall narrow glass, and the other in a broad flat receiver, so that the former may easily stand in its

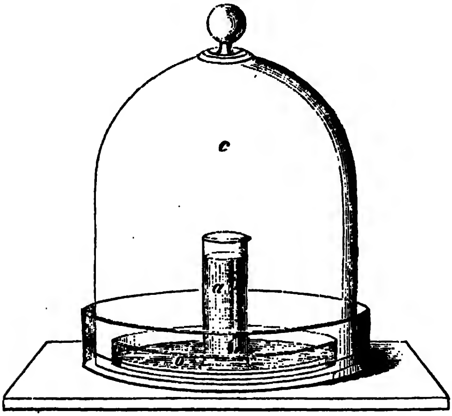


Fig. 69.

Pouchet's Apparatus for showing that Ciliated Infusoria are derived from the Pellicle.

centre[1]. He then encloses them both under a bell-jar, dipping into water; so that the deep solution and the shallow solution may be exposed to the same air under the one bell-glass. And he has ascertained that, at the end of four or five days, with a mean temperature of 68° F, a thick proligerous pellicle is to

[1] See Pouchet's 'Nouvelles Expériences sur la Génération Spontanée,' 1864, pp. 242-247.

be found in the tall glass, and an abundance of ciliated Infusoria; whilst the shallow vessel presents only an exceedingly thin and scarcely apparent proligerous membrane, and not a single ciliated Infusorium. When the conditions are reversed—when the quantity of fluid is much diminished in the tall glass, and very much increased in the shallower one—so as to reverse the relative depths of the solutions in the differently shaped vessels, then the ciliated Infusoria are still found with the deeper solution and the thicker pellicle, and only an abundance of *Bacteria* where the solution is shallow and the pellicle scanty. Remembering that the existence of either ciliated Infusoria, or of the ova of these, to any notable extent, in the atmosphere, is a mere matter of hypothesis, which its advocates have failed to justify; and remembering, on the other hand, the now frequently demonstrated mode of evolution of the ciliated Infusoria in the pellicle from modified aggregations of motionless *Bacteria*,—it must be evident to all that the above-mentioned experiments seem inexplicable if we attempt to explain them by the atmospheric germ-theory, though they are quite consistent with the doctrines of heterogenists. Both solutions are exposed to the same air, and therefore to the same possible source of ova; yet in the solution of the one vessel, after the lapse of a few days, ciliated Infusoria are found; in the other, there are none. Let the conditions of depth of the solutions in the two vessels be reversed, and then again the ciliated Infusoria are met with in

the thick pellicle of the deep solution, whilst not a single one appears in that which is shallow. Only one mode of interpreting such facts seems possible.

Lastly, it was long ago pointed out by Tréviranus and others, as we have before stated, that the kinds of ciliated Infusoria met with in solutions varied with the nature of the solutions themselves. This has since been abundantly confirmed. M. Pouchet has shown that different results are always to be obtained by varying the organic substances, even when these are exposed to the same air and dissolved in portions of the same water[1]. The amount of organic matter existing in the solution will also influence not only the rapidity of appearance, but even the kind of or-

[1] M. Pouchet says ('Nouvelles Expériences,' p. 127):—'Bory de Saint Vincent, Tréviranus, Gérard, et Bérard avaient assuré d'après leurs expériences, que, quand on mêlait ensemble deux liqueurs fermentescibles différentes, on obtenait de ce mélange des êtres organisés qui différaient de ceux que produisait chacune des liqueurs separées.' Again, he says:—'Beaucoup de substances organiques que nous dénaturons pour nos besoins, donnent fréquemment naissance à des productions spéciales parfaitement définies et qui ne croissent nulle parte ailleurs. . . . Les *Mucor pygmæus* et *elegans* ne se développent que sur les aliments qui commençaient à se putréfier et sur la colle sèche; le *Sporendonema casei* ne vit que sur le fromage; le *Chætomium chartarum* sur les vieux papiers qui s'altèrent; le *Sporotrichum ruberrimum* envahit le drap pourri; le *Torula muralis* n'a encore été observé que sur les murailles recrépies. Qui pourrait dire, ainsi que s'écrie M. Fée qui a rassemblé ces faits, où étaient les spores de ces végétaux avant, que l'industrie humaine n'eut donné lieu à ces produits?' (p. 183.) Hundreds of such facts might be cited, so that, as M. Trécul suggests, if it were quite true that all these organisms were derived from spores which pre-existed in the atmosphere, our powers of locomotion might be to a certain extent impaired!

ganisms which reveal themselves; and, similarly, when solid portions of organic matter are immersed in solutions, different results are produced by their immersion at various depths [1]. Abundant evidence of such facts may also be gathered from what has been stated concerning my own experiments. Again, the boiling or not of the organic solution, as we have seen, has a very great influence over the kinds of organisms, as well as over the rapidity with which they appear [2]. It has, moreover, been ascertained that differences in the amount of heat and electricity, and in the kind and degree of light, which are allowed to operate upon the various fluids, are all more or less influential, and exercise a most undoubted influence over the kinds of organisms that are to be met with in different cases. So that the amount and kind of modification which is capable of being brought about in the living forms that are to appear in different infusions made with the same water and exposed to the influence of the same air, are of such a nature as strongly to encourage the belief that such living forms cannot to any appreciable extent be derived either from the air or from the water.

In illustrating this part of the argument which has reference to the development of organisms in solutions exposed to the air, we have purposely laid most stress upon the mode of origin of the ciliated Infusoria rather

[1] See Pouchet's 'Hétérogénie,' pp. 154–159.
[2] See also Pouchet, loc. cit., pp. 148–150.

than upon that of microscopic Fungi or Algæ. We have done this for several reasons. In the first place, the germs of Fungi are very minute. Many of them are merely small, spherical, translucent, but structureless bodies, so that it is often a matter of difficulty, on microscopical evidence, to decide between them and other corpuscles which, though they may present a similar appearance may really be quite different in nature. Again, the origin of Fungi and of Algæ in organic and other infusions, by a process of Archebiosis, had already been established[1], so that we knew more about the possible modes of origin of these than concerning the modes of origin of Ciliated Infusoria. And lastly, because even M. Pasteur himself is unable to say that he has found amongst his particles obtained from the atmosphere many dried bodies of ciliated Infusoria, either in their ordinary or in their encysted condition—or even the ova of these organisms. All are agreed that such things are only exceptionally met with amongst the débris obtained from filtration of the atmosphere, and no one has yet hazarded the opinion that such ciliated Infusoria are capable of originating from aught else derived from the atmosphere but the revived though previously dried bodies of such organisms, or from their ova. No one has propounded the theory that ciliated Infusoria are derived from invisible germs, or from ova other than those of known size and appearance.

[1] By Experiments recorded in Chaps. ix. and xi., and in *Appendix* C.

The facts, therefore, stand in this way. On the one hand it is asserted that pre-existing germs are omnipresent, and that they are the precursors of all the living things which teem in infusions and on all varieties of organic matter in a state of decay. Of these living things, by far the most common and widely dispersed are *Bacteria*; and therefore the hypothesis of Panspermism would require that they should exist most abundantly in the air. But experiments, yielding results of the most indubitable nature, have been performed by Dr. Burdon Sanderson and also by myself, showing that living *Bacteria* or their germs, whether visible or invisible, do not exist to any appreciable extent in the atmosphere[1]. On the other hand, evidence just as convincing goes to show that Archebiosis takes place at the present day—that *Bacteria* are constantly arising *de novo*. And although a recognizable number of the reproductive particles of common Moulds and other microscopic Fungi do exist in the atmosphere, testimony of the most conclusive nature also exists concerning their independent origin. Such Moulds have been proved to be capable of arising *de novo* within closed flasks, whilst every stage of their heterogenetic origin from the constituents of the 'pellicle' can also be easily watched. Similar modes of origin have also been established for Amœbæ and Monads, which are, moreover, not more appreciably represented in the atmosphere than the protean forms of Ciliated Infusoria[2].

[1] See pp. 5-7.

[2] See vol. i. p. 443.

And whilst collateral evidence of all kinds points to the conclusion that the presence of these Ciliated Infusoria in various infusions is explicable only on the supposition that they have been derived from the 'pellicles' which form upon such infusions, every stage of their heterogenetic origin may also be watched with the greatest ease by the microscopist.

Thus although evidence of the most varied and conclusive nature concurs in attesting the frequency with which processes of Archebiosis and Heterogenesis occur, no facts are favourable to the mere assumptions of the Panspermatists, whilst the hypothesis upon which they rely is cumbersome, unwieldy, and now utterly unnecessary.

CHAPTER XIX.

HETEROGENESIS IN HIGHER ORGANISMS.

Nature of Life in Subordinate Living Units. M. Turpin on Milk Globules. Their Conversion into Fungus-germs. Heterogenesis in higher Plants. M. Trécul's observations. Conversion of Crystalline Masses into Living Germs. These processes free from uncertainty. 'Muscardine' of Silk-worms. Its Nature and Mode of Origin. Views of M. Guérin-Menéville. Empusa in Flies. Prof. Cohn's views as to Mode of Origin of. Germ Theory of Disease. Different Interpretations of Facts. Development of Bacteria in Blood of Man. Their Modes of Origin. Vegetable 'Blights.' Views of older Botanists. Presence of Independent Organisms within interior of Plants. Similar Organisms discoverable within Animal Cells. Heterogenetic Developments of their Granules. Mode of Origin of Bacteria in Epithelial Cells. Abundance of Organisms upon Mucous Membranes. Vegetal Parasitic Diseases of the Skin. Possible Modes of Origin. Presence of Fungi upon and within internal tissues of Animals. 'Pébrine' in Silk-worms. Presence of Psorosperms. Their Nature and Modes of Origin. Spread of the Disease.

Mode in which Panspermists explain above-mentioned Facts. No independent Evidence in favour of their Views. Theory of Contagion. Similar influence of living and not-living Contagia. Contact-action *versus* direct Multiplication. Evidence in favour of Contact Theory. M. Davaine's Experiments. Other similar Evidence. Origin and Spread of Local Parasitic Diseases. Inoculation Experiments. Inconsistencies of Evolutionists in adopting a Panspermic Doctrine.

WHEN the functional processes in organs have come to an end in dead animals and plants, there gradually supervenes throughout the body a cessa-

tion of all those complex molecular movements which go on within, and essentially constitute the life of the ultimate constituents of their several tissues. And as the essentially vital changes (or molecular movements) diminish, so do the ultimate molecules of the living tissues begin to undergo rearrangements and decompositions.

We have already endeavoured to show, by cogent experimental evidence, that when organic matter decays or putrefies, a double process of composition and decomposition invariably occurs: the complex organic substances partly break up into simpler binary compounds, during which the previously locked up forces become instrumental in bringing about new synthetic changes among other constituents of the organic matter; and the new products appear as specks of living matter, which gradually grow into *Bacteria*, *Torulæ*, or other simplest forms of life. 'Vital' processes thus lapse into ordinary chemical processes; and thus in turn do these chemical processes again give birth to 'vital' combinations. We now have to refer to this and other modes by which independent living units may arise in the bodies of dead or living organisms.

Whilst it may be possible for heterogenetic changes to take place in some part of the body, even of a healthy animal, provided the intimate vital movements and changes in that particular part are much altered, either accidentally or by the effects of local disease; it becomes much more common for such

changes to occur in various parts of the body when the general 'vital powers' are lowered by disease[1]. And, for a similar reason, heterogenetic changes take place still more freely when the organism itself is dead, and when its component parts are left to struggle on under the most adverse circumstances, until the little-remote period when death overtakes them also[2]. Then in all parts of the dead organism there is a bursting-forth into new life. Myriads of *Bacteria* and Fungus-germs are born from their parent fluids, though all this is hidden from our ordinary view, and its effects only become manifest when the ever-varying forms of 'mould' and 'mildew' appear and flourish on the surface of the previously living aggregate[3].

For the most part I intend to confine myself to the consideration of the mode of origin of these lowest organisms within the substance of higher plants and animals. I do not propose to enter into the question of the possibility of the independent origin of any of the higher parasitic Entozoa. The occurrence of these parasites was formerly regarded as one of the strongest points in favour of the doctrine of Heterogeny. But the investigations of numerous helminthologists have done much to remove very many of the difficulties which were formerly regarded by Müller and others as almost impossible to be explained on the supposition

[1] See p. 190. [2] See vol. i. p. 110.
[3] See Prof. Grant's 'Tabular View, &c., of Recent Zoology,' pp. 5 and 91.

that these organisms had been derived in the ordinary way from ova. The migrations and transformations of entozoa in the bodies of different animals, and our knowledge of the mode in which the embryos of cystic and nematoid parasites are enabled to penetrate the tissues, clear away many of these old difficulties. It must be confessed, however, that the reality of such new facts does not veto the possibility of the occasional independent heterogenetic origin of some of these organisms. I will merely state that such a mode of origin is still affirmed by Dr. Gros[1] and others, but having made no special observations on the subject, I purpose deferring its consideration till some more suitable period.

It might be deemed probable that, if heterogenetic changes occurred at all in higher animals, they would be most prone to take place in some of the fluid or semi-fluid secretions; or else in some of those tissue-elements which are constantly bathed with albuminoid fluids—either on the external surface of the body, or on some internal surfaces. And this is found to be the case. No better, longer known, or more generally neglected instance can be alluded to than the transformation of milk-globules, under certain conditions, into large Fungus-germs, which speedily vegetate into a kind of *Penicillium*.

This remarkable transformation was described by

[1] 'Bullet. de la Soc. de Nat. de Moscou,' 1847.

M. Turpin[1] thirty-four years ago, in a paper read before the French Académie des Sciences; but it has been for the most part disbelieved or unheeded by many who ought to have satisfied themselves by actual observation as to the truth or falsity of what had been recorded. With some rare exceptions, this seems to have been neglected, though the few who have looked for themselves have been able, in all important respects, to confirm M. Turpin's statements.

When some milk is placed in a small vessel, to the depth of about two inches, the larger milk-globules soon begin to collect on the surface of the fluid. After twenty-four hours or more (the milk being protected from dust by an inverted glass), the surface is found to be yellowish and smooth—constituting the most superficial stratum of a layer of cream, the under portions of which are of an opaque white colour. When reflected, this is found to lie on the surface of a bluish-white whey containing soft flakes, which, on microscopical examination, are ascertained to be composed of precipitated casein in a finely granular condition, mixed with small milk-globules and multitudes of active *Bacteria*. In this condition, it has a sour odour and an acid reaction. The white stratum of cream, immediately above, is composed almost wholly of aggregated and more or less unaltered milk-globules, mixed with myriads of *Bacteria*. But it is in the superficial yellow stratum more

[1] 'Ann. des Sc. Nat.' 1837 (Zoologie), t. viii. p. 349.

especially that the milk-globules are found to be variously altered, and in which some are being metamorphosed into Fungus-germs. To recognise this satisfactorily requires great care and patience, and it is only possible by making an examination of specimens in which the transformation is in its earliest stages. After even a few hours, owing to the very rapid growth and repeated branching of the *Penicillium*-filaments, the superficial stratum is permeated by them in all directions; and they are mixed up soon afterwards with the large conidia which the filaments are constantly throwing off, and which germinate in their turn.

The superficial stratum should therefore be examined at the period when the globules are just beginning to bud into filaments, or, better still, the method originally recommended by Turpin may be adopted. A drop of distilled water should be placed upon an ordinary glass microscope-slip, and a small quantity of the as yet unaltered cream should be added so as to disseminate its globules through the fluid. A covering glass may then be applied, and allowed to float somewhat freely on the fluid. After a microscopical examination of the specimen, with the view of ascertaining the state of the globules and the absence of all apparent Fungus-germs, the specimen should be carefully transferred to a damp chamber which is thoroughly saturated with moisture—so as to prevent, as much as possible, the evaporation of the fluid from beneath the covering glass. Or else the drop of water containing milk-

globules may be left without a covering glass, if the slide is placed in a small chamber thoroughly saturated

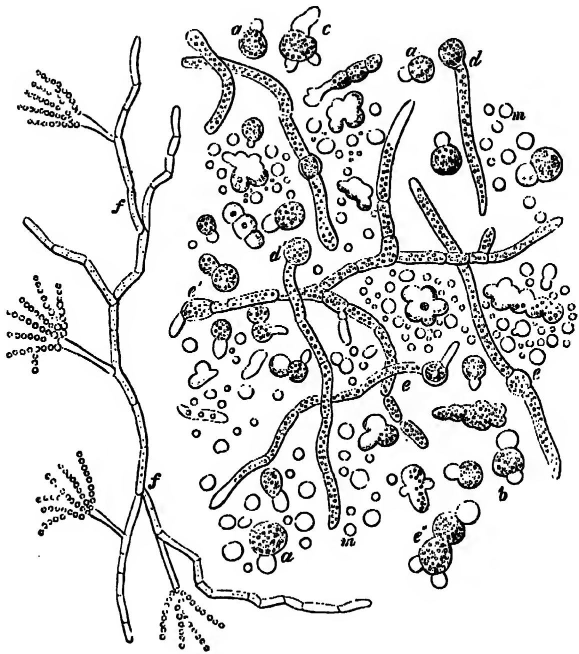


FIG. 70.

Conversion of Milk-globules into Fungus-germs. (Turpin.) (× 450.)

m, m. Unaltered Milk-globules.
a, a. Milk-globules which have become granular, throwing out a single bud.
b, c. Globules with two and three buds.
d, e. Rudimentary mycelial filaments attached to globules.
d′, e′. Filaments in more advanced condition.
f. Perfect filaments of *Penicillium* less highly magnified.

with moisture[1]. The specimen may then be examined from time to time with a $\frac{1}{12}$ objective; and in the course of from twenty-four to seventy-two hours, according to the temperature, such changes will have occurred in many of the milk-globules (ranging from $\frac{1}{10000}''$ to $\frac{1}{2000}''$ in diameter) that they may be seen to have assumed a less refractive and more distinctly vesicular appearance, and to be giving birth to one, two, or even three buds, from their periphery, which speedily grow into large dissepimented mycelial filaments[2]. All this was definitely described by M. Turpin. In speaking of the milk-globules, he said: 'Lorsqu'ils se trouvent livrés à eux-mêmes et placés dans les circonstances favorable à la continuité de leur existence, ne tardent pas à se gonfler, à prendre souvent la cirforme irrégulière d'un petit topinambour microscopique, et à germer par plusieurs côtés à-la-fois, de la même manière que germent les seminules vésiculeuses des Confervées; des Mucédinées des Champignons, et des vesicules polliniques.' Or, as I have also been able to ascertain, other globules, 'au lieu de commencer par prendre un développement irrégulier deviennent ovoïdes, puis allongés comme de petits bouts de cylindre, et dans ces divers états, ou plutôt sous ces formes modifiées,

[1] In a small wide-mouthed, stoppered bottle, for instance, lying on its side, and containing a little water.

[2] Other globules become fused together so as to form large irregular masses of various kinds. Multitudes of *Bacteria* also appear amongst the globules.

poussent des bourgeons par l'une ou par les deux extrémités à-la-fois, et produisent également le même *Penicillium glaucum*' (loc. cit., pp. 340, 342). The fact that so many corpuscles undergo a similar change beneath the same covering glass, that these changes take place in corpuscles which are so large as to be most easily observed, and that all stages may be detected between apparently unaltered milk-globules and the large Fungus-germs into which they are transformed, make these observations absolutely convincing to any one who has once witnessed them[1]. They therefore become typical of many other changes which may take place, but in which all the stages of the transformation cannot be so easily watched[2].

[1] And yet, in opposition to the investigations of M. Turpin, extending, as he says, over more than six weeks, and the positive statements which he was able to found upon them, one of our most influential authorities on such subjects is content to offer the following somewhat loose criticism:—'Without laying too much stress on the difficulty of following up the development of a single globule amongst a multitude, there can be no reason why spores of *Penicillium*, or at least particles capable of reproducing it, should not be present in the milk as well as the *oïdium* in diabetic urine. And though the true spores are of considerable size, it is more than probable that many moulds—as, for instance, such as grow on paste, decaying meat, vegetables, &c.—assume on their first development a form very different from that of the full-grown plant.' ('Introd. to Crypt. Bot.,' Berkeley's, p. 260.)

[2] Since the above has been in type, I have ascertained that heterogenetic transformations may be much more easily seen in a minute portion of Neufchatel cream-cheese. By placing a portion, about the size of a pin's head, upon an ordinary glass slip, moistening it with distilled water, and spreading it into a thin film, the changes which it undergoes can be readily watched. When kept in this moist uncovered state in a damp chamber at a temperature of 65° F, I have found that at the expiration

Left to itself, the whole surface-layer of milk in a short time becomes densely interwoven with Fungus-filaments; and multitudes of the conidia which they are continually throwing off are sown amongst them. Soon a white mildew may be seen even with the naked eye sprouting up from all points of the surface, and after a time it becomes covered with a perfect forest of *Penicillium glaucum*[1].

It seems probable, moreover, that somewhat similar changes may occasionally take place within the mammary ducts themselves when, owing to some diseased condition of the gland, the milk is long retained. A specimen of milk was sent to M. Turpin by M. Lassaigne which had been taken from a cow whose mammæ were somewhat inflamed and engorged; and this was found to contain a very large amount of fungus-mycelium. It is said:—'Ce lait sortait des mamelons ou trayons sous la forme de petits flocons d'une beau blanc et d'un aspect entièrement cotonneux.' And, on microscopical examination, these flakes proved to be

of forty-eight hours, nearly one half of the fatty-looking mass had actually undergone segmentation into Fungus-germs, many of which had in their turn grown out into well developed filaments.

[1] In view of the observations just detailed, it becomes a most significant fact that precisely the same kind of fungus is apt to spring up on all sorts of organic matter when it begins to undergo processes of decay. As Turpin says, one may now conceive that 'independemmant des moyens reproducteurs secondaires, tels que ceux de la seminale et de la bouture, le *Penicillium glaucum* peut se montrer avec une étonnante profusion partout ou se recontrent les globules producteurs de la matière organiques.'

masses of dense interlaced fungus-filaments, and of more or less altered and agglutinated milk-globules. So that one can only conclude, as Turpin[1] suggests, that 'les globules laiteux, arrêtés et accumulés dans les voies d'une mamelle surirritée et engorgée, y avaient produit lorsqu'ils vivaient encore, les filaments byssoïdes et mucédinées comme cela se voit chez les globules laiteux abandonnées à eux-mêmes sous l'influence de l'air et de l'oxygène[2].'

This metamorphosis of the milk-globule may be most suitably compared with other heterogenetic changes which have been made known more recently by M. Trécul, one of the most distinguished botanists in France, as occurring within the tissues of many flowering plants and shrubs.

It may easily be imagined that the aerial leaves of ordinary plants and trees are not favourably situated for the occurrence of evolutional changes in their interior. The living matter of which they are composed is exposed too much to the drying influence of the air, and to other adverse conditions, to enable it to give birth to anything save Fungus-germs, or similarly low organisms. And as for their internal tissues—the fluid or semi-fluid portions of these being cut off from the free access of air, and also distributed for the

[1] 'Mém. de l'Acad. des Sciences,' 1840, t. xvii. p. 232.

[2] M. Turpin, moreover, suggested that a similar germination of the milk-globules might take place occasionally in the mammary ducts of women after child-birth, when the exit of the milk is delayed and the breast is irritated and inflamed.

most part in small quantities through numerous separate and more or less closed cellular compartments—they are little prone to undergo any save the lower modes of organic evolution. But, as we have learned from the investigations of M. Trécul, and from the observations of many other workers, *Bacteria* may be produced in abundance in these situations, and so also may low fungoid organisms.

When M. Trécul placed some fragments of *Apocynum* in water, in order by maceration to isolate the laticiferous vessels, the latex within the latter at first underwent its accustomed alterations in appearance. The small globules which it usually contains united either into larger globules, or else into masses of a more or less homogeneous character. At a later stage all this latex had undergone a new change; it had become finely granular, and there only remained here and there, as relics of the former condition, minute portions of the old homogeneous material. M. Trécul says[1]:—'This was of itself a sufficiently singular occurrence. But my surprise was great when, after having placed these laticiferous vessels in contact with iodine and sulphuric acid, I saw their whole contents become of a deep violet colour, whilst the little masses of latex which had not undergone this last change, and which were enveloped by portions of the juice that had become thus finely granular, remained uncoloured, or else had assumed the yellow colour which iodine

[1] 'Compt. Rend.' (1865), t. lxi. p. 158.

frequently communicates to the latex. . . . Having then directed my attention to the fine newly-formed granules, I perceived that they were more extended (plus étendues) than they at first sight appeared, since

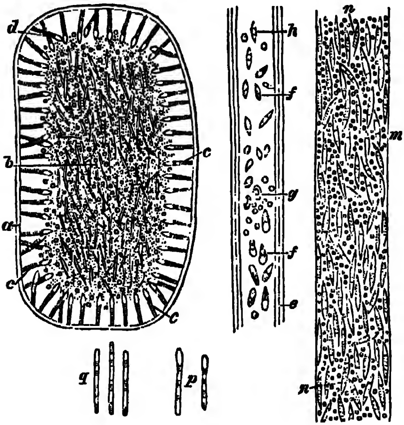


FIG. 71.

Origin of *Amylobacter* within cells and laticiferous vessels of Plants. (Trécul.) (× 520.)

m. Portion of a laticiferous vessel of *Amsonia latifolia* whose contents have been transformed into fusiform *Amylobacters.*

a. Medullary cell of *Ficus carica* filled with granules and fusiform *Amylobacters* (*b*), and having other large capitate *Amylobacters* upon its internal wall (*c*).

e. Portion of thick-walled bark-cell containing different forms of *Amylobacter.*

p, q. Other forms irregularly stained by iodine.

each violet spot was, in certain vessels, only the termination of a little oblong body which was composed of two or several cells, and was either colourless or slightly stained yellow. Elsewhere, other cells of this

little organism were more feebly-tinted violet, or else they were all alike intensely-tinted.'

A closer examination of these bodies has shown them to be organisms which differ considerably in size and shape on different occasions. They present themselves as very minute globular bodies; in the form of small cylinders, either single or capitate; as larger elliptical corpuscles which may elongate into fusiform organisms about $\frac{1}{5000}''$ in length; or, lastly, as corpuscles with a projecting shoot. Some are motionless, and others display slightly undulating movements. These bodies, from becoming variously stained by iodine, show that a starchy matter is produced during their metamorphosis and growth. Owing to this fact, and on account of the resemblance of many of them to *Bacteria*, they have been included by M. Trécul under the name of *Amylobacter*.

In other vessels such a change, instead of having been effected throughout the whole vessel, was seen to be still in progress. 'One part of the column of latex had become purple from the action of the iodine and sulphuric acid, whilst another had become yellow; but from the one to the other tint every transition was to be seen. . . . Some other unbroken vessels were very instructive, inasmuch as their latex, not being modified to the same extent, assumed a yellow colour under the influence of the re-agents; only corpuscles (cells) of a violet colour were dispersed throughout its interior, and they were often quite separated from

one another.' M. Trécul adds:—'*It is important to note that I did not find any of these little organisms dispersed through the liquid which surrounded the laticiferous vessels.*' To account for the presence of the organisms under such circumstances, only two suppositions seem possible. As M. Trécul says:—'Either they are born from germs proceeding from without, or else they proceed from a modification of the elements of the latex. If they owe their origin to pre-existing germs, how are these germs introduced by thousands throughout the whole length of vessels filled with a dense fluid so consistent as to be no longer able to flow, and to such an extent as completely to substitute themselves in the place of the juice itself? How can one conceive, whilst admitting such an invasion of germs, that small islets of latex should have remained intact here and there, and should have been able to resist this invasion which pressed round them on all sides? Is it not at least as probable that these organisms may have been born from a transformation of the latex?'

In a subsequent communication made in September, 1865, M. Trécul[1] reported that he had confirmed the results previously arrived at by fresh observations upon similar plants, and also upon others belonging to different families. In one of these, *Ficus carica*, he had even discovered similar starch-bearing fungoid organisms within the completely closed cells of the medullary tissue,—a fact which seemed to make

[1] 'Compt. Rend.' t. lxi. p. 432.

their mode of origin even more certain than it had been before. In some situations they were something like tadpoles in shape, whilst in others they were cylindrical, or only very slightly attenuated towards one extremity. But M. Trécul tells us—and no one is more competent to pass an opinion on such a subject—that 'the appearance of these little plants within closed cells, occupying their natural situation in the very middle of the medullary tissue, negatives all ideas as to the introduction of germs from without.' And he has even seen similar organisms produced within fibre-cells of the bark which had already become notably thickened, in *Asclepias cornuti* and also in *Metaplexis chinensis*. (See Fig. 71, *a*, *e*.)

But M. Trécul is able to add another proof even more striking than any we have hitherto mentioned. He has actually seen a crystalline mass undergo modifications, and become itself converted into an *Amylobacter*. There exists, he says, in the bark of the common Elder, and in that of plants belonging to different families, such as *Solanaceæ* and *Crassulaceæ*, a number of cells which are filled with little tetrahedrons having slightly unequal sides. These cells may be isolated, or they may be grouped in contact with one another, and in longitudinal series. The cell-walls sometimes become partly absorbed, so as to form intercommunicating lacunæ, and it is within these that the enclosed tetrahedrons become converted into starch-bearing organisms. M. Trécul says:—'Since my observations

in 1860, I had become aware that corpuscles, which coloured violet under the influence of iodine, frequently replaced the tetrahedrons after putrefaction; but, at this period, I had not seen the transition from the one to the other. I was more fortunate this year. I have seen the tetrahedrons themselves, containing amylaceous matter, forming columns, tinted with the most beautiful violet colour. I have seen the tetrahedrons become elongated at one of their angles, and pass gradually into these curious little plants, by producing a cylindrical outgrowth. In this case, the rounded or still angular tetrahedron represented the bulb, but the tetrahedron occasionally became completely obliterated, and left in its place only a little fusiform or cylindrical vegetal organism.'

This is indeed an example which, in point of certainty and freedom from possible sources of error to a skilled observer, seems almost unsurpassable. If a crystalline mass of matter is seen slowly to alter its form and become bodily converted into a vegetating organism, one could not have evidence of a more convincing nature. Only one explanation of such a fact is possible—hence M. Trécul is quite entitled to say[1]:—'De tous les faits qui précèdent, il résulte que *la matière organique contenue dans cértaines cellules peut se transformer, pendant la putréfaction, en corps vivants de nature très-différente de l espèce génératrice.*'

[1] Loc. cit., p. 435.

These transformations of the particles in the latex of plants, and the somewhat similar transformation of milk-globules, are most important and typical, since the change takes place in comparatively large masses of matter which can be watched with ease. It takes place, moreover, in masses which, although they are the products of living bodies, can scarcely themselves be said to be 'living[1].'

In the blood of animals we have another highly nutritive fluid which might be supposed to be capable of giving birth to independent living things under certain conditions. It seems, however, to undergo such changes more frequently in the lower forms of life than in the higher. Amongst insects, several instances may be mentioned in which simple organisms appear to be born from the fluid constituents of the blood, or else produced by modifications of some of its already existing solid elements.

This seems to be the case with 'Muscardine,' the disease which formerly committed such fearful ravages amongst the silk-worms of France.

[1] There is, perhaps, most room for doubt, in the latter respect, concerning the particles in the latex; and these, being probably poor in nitrogenous materials, evolve into very small and simple organisms. The milk-globules, however, have a highly nitrogenous composition, owing to an admixture of albuminoid constituents with their fatty elements—a combination which seems especially favourable for the occurrence of evolutional changes. The milk-globules accordingly are seen to produce large specimens of *Penicillium* of a remarkably vigorous growth.

It was ascertained by Bassi in 1835, that one of the most prominent features of this disease was the presence of a Fungus which at first increased and multiplied within the body of the living animal, and, after the death of the worm or moth, made its appearance externally—coming through the skin in various places as a whitish powdery growth. It was afterwards ascertained by M. Audouin and others, that the disease was not confined to the silk-worm and its moth, since it could be communicated directly by inoculation to many other species of Lepidoptera; and it could also be engendered quite easily in these and in the silk-worm by shutting them up and feeding them for a time in close damp bottles or boxes. The possibility of inducing a 'spontaneous' outbreak of this contagious disease was always within reach of the experimenter, even in districts which were, so far as all previous knowledge went, wholly untainted. In this respect, muscardine was found to be similar to typhus fever[1]. The question arises, however, whether, in such cases of apparently 'spontaneous' origin, the unhealthy conditions merely induce a state of the blood and body generally in which omnipresent, although unknown, spores are enabled to develop; or whether the state of the

[1] Muscardine, however, is undoubtedly associated with the development of a fungus in the blood; whilst in typhus fever no lower organisms are known to be produced. Their non-existence in the latter disease is further testified by the large number of recoveries. On the other hand, muscardine is invariably fatal. As Audouin says, 'dans tous les cas le résultat est le même; aucun de ceux qui sont attaqués n'échappe.'

blood thus initiated does (as it seems to do) suffice to give rise to the germs and fungus-growths which afterwards constitute such all-important elements of the disease. The former is the view which has been most widely adopted: and yet two of the ablest writers on muscardine, after the most careful investigations with reference to this very point, came to the opposite conclusion. These observers, who adopted, in its entirety, the belief in the possibility of engendering muscardine *de novo*, were M. Guérin-Méneville and M. Robinet. The conclusions of the former especially were based upon positive and apparently unambiguous observations.

The blood-corpuscles of the silk-worm during life are elliptic or more or less elongated, but after death they are always found to be spherical. When a little blood is abstracted from a healthy worm, the corpuscles are elongated at first, though they speedily assume the spherical condition; and, when in this latter state, they begin to exhibit amœboid protrusions, although such changes of shape are never seen in healthy blood-corpuscles immediately after they have been drawn from the body. M. Guérin-Méneville observed that in dead silk-worms, and also in the cases where blood had been drawn from living animals during the very early stages of the disease, the spherical amœboid corpuscles contained much larger granulations than usual; and that some of them tended towards the periphery of the corpuscles, from which they ultimately made their exit. These little bodies were ovoid, and from

$\frac{1}{25000}''$ to $\frac{1}{30000}''$ in length. They moved about in the serum of the blood more actively than could be ac-

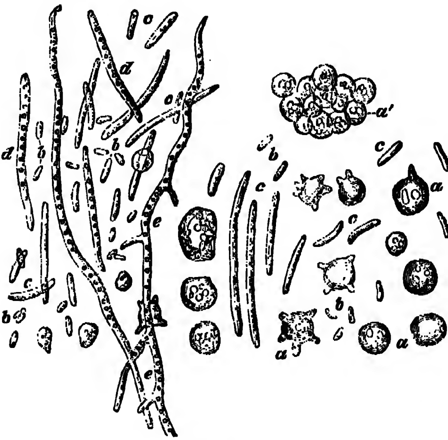


FIG. 72.

Illustrating the Development of *Botrytis Bassiana* in the Blood of Animals suffering from Muscardine. (Guérin-Méneville.) (× 400.)

a, a. Spherical and amœboid blood-corpuscles containing large particles.
b, b. Ovoid particles in the free state.
c, c. Germs of *Botrytis* supposed to be derived from such corpuscles, which gradually grow (*d, d*) into long and simple, and subsequently (*e, e*) into branched, Fungus-filaments.

counted for by mere Brownian movements. They gradually increased in size; and, even two or three days before the death of the sick worms or moths, many of them had become so elongated as to be easily recognisable as rudimentary fungus-filaments[1]. Along with

[1] In reference to the different conditions under which the same phenomena may be witnessed, it should be remarked that the fungus can

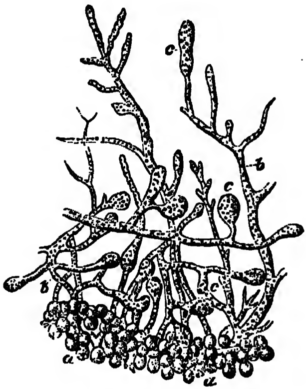
this characteristic feature of muscardine, another very significant peculiarity invariably showed itself, *i. e.* a well-marked acidity of the blood, instead of the neutral condition which is invariably met with in the healthy worm or moth[1].

The fungus, continuing to grow through the tissues of the animal in all directions, soon causes its death; and, after twenty-four hours or less, it may make its appearance externally through the thinnest portion of the dorsal integument, and through the respiratory spiracles, in the form of delicate snow-white tufts of fungus-mycelium. These continue to grow rapidly, and soon produce multitudes of minute spores, by means of

always be developed in dead animals. According to Robin, this was shown by M. Johannys in 1839; for, as he says, 'il a pu faire développer ce végétal sur des Vers à soie morts, placés dans des conditions favorables à la fermentation et *hors de toute communication avec des lieux infectés de Muscardine.* La moisissure se développe aussi bien sur l'animal mort que sur les animaux inoculés de leur vivant, et le végétal obtenu par inoculation est identique avec celui, que se développe sans cause directement connue.' Whilst, according to evidence given by M. Guérin-Méneville in 1851, 'la muscardine sporadique est une terminaison naturelle de l'existence du ver à soie, maladie qu'il est impossible de prévenir d'une manière absolue.' (Loc. cit., pp. 574 and 603.)

[1] From the evidence now in our possession, it is not perhaps possible to say whether the acidity precedes the appearance of germs, or *vice versâ.* We do know, however, from the experiments of Dutrochet, that a mixture of egg-albumen and water will remain unchanged for months, but will always develop fungi within a few days after it has been slightly acidified. ('Ann. des Sc. Nat.,' 1834 (Botan.), p. 34.) Again, milk always assumes an acid reaction before its globules begin to undergo transformation into *Penicillium*-germs.

which the disease may be disseminated amongst those silk-worms which, owing to bad hygienical conditions or other causes that may have lowered their vitality, are



More developed form of *Botrytis Bassiana*, as it grows through the tissues of Animals suffering from Muscardine (Audouin). (× 400.)

at all predisposed to initiate the particular morbid changes by which it commences[1].

[1] It is admitted on all sides that such a predisposing cause is necessary. Thus Robin says:—'Il ne suffit pas que les spores arrivent à la surface du corps des larves ou même dans la intérieur; il faut encore, pour qu'elles puissent agir sur l'animal, que ses humeurs présentent des conditions favorables à leur germination. Si les Vers exposés au contact et à la pénétration des spores sont placés dans les circonstances d'alimentation très favorables, il se pourra probablement que les humeurs dont les principes se renouvellent rapidement, ne présentent pas ces conditions; on dit alors que l'animal résiste par son énergie propre. C'est la certainement, dit Robinet, la ressource la plus sûre et la plus facile à créer par la magnanier. C'est ainsi que dans des invasions désastreuses

In its epidemic form, muscardine is most prone to occur at the season when the worms are just about to pass into the chrysalis condition. At this critical period, unknown influences are apt to operate upon them which produce abnormal and specific nutritional changes; and then the disease breaks out like a pestilence, attacking almost all the worms of a given locality.

Again, it is well known that the growth of a mould (*Empusa*) frequently seems to prove fatal to flies in autumn[1]. This subject has been investigated by Professor Cohn, who came to the opinion that the parasitic growth commenced by an independent development of Fungus-germs in the blood of the sickly animal. He does not believe that the disease is incited by the mysterious introduction of Fungus-germs into their circulating system from without. He sums up by saying ('Hedwigia,' 1855, p. 59) that 'the influence of the spores of *Empusa* in the appearance of this fungus, and of the disease in flies, is by no means evident, since the genesis, the chemical and optical characters of the numberless free cells in the blood, the absence of a special expanded mycelium, and, above all, the whole

de la muscardine une foule d'individus échappent, non pas à transmission, ainsi qu'on le dit, mais bien au développement des spores. Voilà pourquoi beaucoup d'observateurs ont contesté la transmissibilité de la Muscardine d'un individu à un autre.' ('Végétaux Parasites,' 1853, p. 582.)

[1] This mould will always appear on a dead fly which is allowed to float for a few days on the surface of water.

history of the development, seem to concur in favour of the origination of the cells of the *Empusa* from the diseased blood [1].'

Although much has been written of late years with reference to the existence of germs of fungi in the blood of man, and as to their production of many of the most serious diseases to which he is liable, I have elsewhere [2] noted what slight support there is for these various statements, and to what a large extent different kinds of evidence tend to contradict them. As a matter of fact, such organisms are not to be found in the blood of living persons except in certain rare affections; and doubtless many of the alleged cases are to be explained by the altogether unwarranted assumption of observers that mere particles, which are so commonly met with in the blood, are fungus-germs—specimens of the so-called 'micrococci' of Hallier [3].

[1] Of course it is not denied that such a disease is capable of being spread by contagion: far from it. We believe that this does occur. The establishment of the mode by which contagion is communicated cannot, however, as some have appeared to think, dispose of the more general question. (See Prof. Huxley's Inaugural Address to British Association, 'Nature,' 1870, No. 46, p. 405.)

[2] See *Appendix* E, pp. cxix-cxxvii.

[3] But it must always be remembered that there are particles and particles. Those which are potential fungus-germs cannot be discriminated by the microscope from normal blood-particles; and even if some of these particles were seen to develop into rudimentary fungi, it would by no means prove (as Prof. Hallier and his disciples would suppose) that they came from fungi—in the face of what we now know concerning the metamorphosis of milk-globules and other organic products.

There are, indeed, weighty reasons why such a fluid as the blood should not, amongst very highly organised creatures, easily give birth to heterogenetic products during the life of the individual. It is not a mere excretion like the milk, but the most nearly vitalised of all the fluids of the body, and subject to constant changes from moment to moment in all the tissues: so that, as long as the organism lives, the united molecular activities of the various tissues are continually influencing this all-pervading fluid, and tending to maintain its ordinary characters[1]. But when death is approaching, these united activities become weaker and weaker, and changes may begin to take place in the blood more closely resembling those which occur in organic fluids out of the body[2]. It is quite conceivable, also, that the changes which occur in the blood in certain febrile diseases—more especially when they are associated with a very high temperature—may be of such a nature as to make the blood more than usually prone to undergo rapid putrefactive changes after death. And the occurrence of putrefactive changes in animal fluids implies the presence of *Bacteria*.

Facts can, indeed, be cited, tending to show that the changes in the blood in these diseases do predispose it to early putrefaction, and that the living organisms

[1] See p. 188.

[2] It is quite certain that refuse fluids, such as urine in certain diseased conditions, may contain an abundance of *Bacteria* and *Torulæ* at the moment when they are passed from the body.

which have been observed soon after death must have been produced *de novo* in the organic fluids themselves. One observation of this kind made much impression upon me at the time, and tended strongly to confirm a then growing belief in the truth of the doctrines concerning 'spontaneous generation.' Having made an autopsy (thirty-two hours after death) nearly three years ago, on a man who died in University College Hospital of rheumatic fever, and in whom an exceedingly high temperature had existed for a few hours before death, I immediately proceeded to examine portions of the brain and membranes with the microscope. The skull had been opened and the brain had been removed in my presence but a few minutes before; when the arachnoid was cut through, and two convolutions were carefully separated which had previously been in close contact, in order to cut off a portion of the delicate network of vessels lying between them. On submitting this to the microscope, the fluid outside the vessels, and also that within, was seen to contain a large number of most actively moving particles. Many of these were mere spherical particles of various sizes, but others were distinct and large *Bacteria* made up of two almost cellular segments; and every portion of the pia mater that was examined showed similar moving particles and *Bacteria*. The brain was then covered by a bell glass, and when portions of the pia mater—again taken from between previously unseparated convolutions—were examined after an interval of twenty-four hours, the large *Bacteria*

had considerably increased in number, whilst the small spheroidal particles seemed to be as plentiful as before. When portions of brain substance from some central parts of the organ were also examined at this time, moving particles and *Bacteria* were seen to exist in the greatest abundance amongst the disintegrated nerve elements, which had probably been poured out from the blood-vessels.

Now, with regard to the origin of those *Bacteria* which were observed in the vessels a few minutes after the brain had been removed from the body, it is, in the first place, perfectly obvious that they must have existed in the blood of these vessels before the brain had been removed and before the skull was opened. *Bacteria* are not produced in any fluids under two or three hours. Their origin could not, therefore, have been due to *Bacteria*-germs derived from the atmosphere, which, on removal of the skull-cap, had in some mysterious way insinuated themselves into the blood vessels. They must either have existed in the blood during life, or else they must have been produced *de novo* in this fluid after death. There is strong reason for disbelieving that *Bacteria* existed in the blood during the life of the individual. I have several times examined the blood of individuals who were similarly affected with this exaggerated form of rheumatic fever, and have always failed to discover any such organisms[1]. It is, therefore, far

[1] Even if they had existed, however, during life, there would still be weighty reasons inclining us to believe that they had been produced *de*

more likely that they had been newly evolved, by reason of changes taking place in the blood after death, in or near the situation in which they were found. It would be impossible otherwise to account for their distribution throughout the brain at a time when the circulation had ceased[1]. This organ is so far, comparatively, from any mucous surface, that even if germs had been able to make their way into the blood-vessels ramifying on their surface (which is in itself altogether a gratuitous supposition), it would be impossible for us to imagine that, in such a short space of time, they could have been able to penetrate into the innermost parts of the brain. Their unceasing movements are extremely slow in reality; and more than this, they are never continuously progressive. They consist either of slow oscillations, or else of short darting movements hither and thither, in which the same ground is frequently retraced. And again, if *Bacteria* in their adult or in their rudimentary state could make their way from the atmosphere through the superficial layers of the mucous membrane so as to penetrate the vessels, why, if it is to be assumed that they do this after the death of an individual, does not the same thing occur during his life, more especially when the mucous membrane of

novo in the blood, rather than that they had been developed from germs which had gained access to the blood from without.

[1] In all probability, if examination had been made, they would have been found disseminated throughout all other parts of the body, just as they were actually found in different parts of the brain. (See *Appendix* E, p. cl, note 1.)

the mouth nearly always contains myriads of them[1]? It cannot be replied that *Bacteria* are unable to live in the blood during life, since the observations of Davaine, Vulpian and others clearly show that they can flourish in the blood of man and also in that of several of the lower animals during their life. There seems then to be no reasonable alternative, and we are compelled to fall back upon the assumption, that the *Bacteria* met with in our observations had been evolved out of the blood plasma and other fluids of the body, just as we have seen that they arise in previously heated organic infusions[2].

During life, and under the influence of all the varied activities of the living body, the plasma of the blood, rich in nutritive materials, is probably giving birth constantly to living particles which speedily develop into leucocytes. These amœboid corpuscles are the organisms into which such new-born living matter invariably tends to develop in the healthy living body[3]. But when death has supervened, then all is changed; the molecular composition of the fluid may have altered, whilst the activities of the tissues which formerly influenced it have ceased to act. It is, however, a fluid still rich in albuminoid materials; and when released from

[1] See pp. 345, 346.

[2] More especially since the more recent investigations of Dr. Sanderson have led him to the conclusion that the blood does not naturally contain either visible or invisible *Bacteria*. ('Thirteenth Report of Medical Officer of Privy Council,' 1870, p. 65.)

[3] See vol. i. p. 226.

all the vital influences of the organism of which it formed part, what wonder is it that the new-born particles which are still evolved should assume the familiar shapes of such units (*Bacteria*) as appear in organic solutions outside the body; or that some of the minute particles which existed in the blood, before death was close at hand, should alter their destination and develop into *Bacteria*, just as the milk-globules develop into large Fungus-germs? It is also quite possible that a heterogenetic change of some kind may overtake red and white blood-corpuscles when they are liberated from the vital influences of the organism in which they have been produced, and also occasionally within the living organism itself. On this subject, however, we have at present very little evidence[1].

[1] White blood-corpuscles are practically young Amœbæ, and there is no saying what changes they may not occasionally be capable of undergoing. *Gregarinæ*, which are so very abundant in the bodies of lower animals, and which are closely allied to Amœbæ, may perhaps in many cases be derived from the transformations of such corpuscles. Again, red blood-corpuscles are very similar in many respects to the chlorophyll vesicles of *Algæ* and *Characeæ*, although the latter are probably much less specialised in composition. But it will appear further on (Chap. XX) that the transformations of such chlorophyll vesicles are often of the most startling description. Quite recently, Mr. Ray Lankester ('Quarterly Journal of Microscopical Science,' 1871) has described a peculiar, though small and simple, ciliated Infusorium which he found in the blood of certain frogs; whilst Dr. Boyd Moss ('Monthly Microscopical Journal,' Oct. 1871) has described a similarly simple Infusorium found in the blood of a Ceylon red deer on several occasions. It seems to me more easy to suppose that such organisms should have arisen by a heterogenetic process, than from 'germs' of delicate, externally existing organisms of this kind, which had not only made their way into the

The two modes of origin of organisms to which we have just alluded may also lead to the presence of *Bacteria* and larger Fungus-germs within the interior of closed cells, both in plants and animals.

Myriads of microscopic fungi belonging to the protean types included under Rust, Smut, Mildew, and Mould [1], are habitual dwellers in and upon the surfaces of living plants, especially when they are in a sickly condition—although others are often found in and upon plants which present no other sign of disease. These particular kinds of fungi are encountered only in such situations, and they recur in similar habitats with tolerably constant forms. The ravages of many of them are matters of no small importance to mankind on account of the very serious damage which they help to produce in our food-supplies. We need only mention the fatal 'blights' which they are apt to occasion amongst our cereals, and those devastating diseases of the vine, the hop, and the potato, in which fungi of this kind appear as the most active agents of destruction. The original mode of origin of these various growths is still involved in doubt and obscurity,

blood, but were capable of flourishing there. Again, Dr. Gros ('Bull. de la Soc. de Nat. de Moscou,' 1845, p. 424) says:—'Le sang d'une mulot nous a présenté des vermicules si nombreux que toutes les vésicules en avaient l'air animées, et si petits qu'ils étaient à peine reconnaissables à 400 diamètres. Le sang des taupes présente souvent le même cas.'

[1] See a useful little book by M. C. Cooke, entitled 'Microscopic Fungi,' 2nd ed. 1870.

although most positive statements have from time to time been made by different observers concerning their heterogenetic origin, by changes similar to those which convert the milk-globule into a *Penicillium* and the globules of many plants into *Amylobacters*[1]. Thus, in speaking of one of the commonest blights, that produced by *Uredo*, M. Turpin says[2]:—'Il m'est bien démontré par un grand nombre d'observations, faites sur diverses plantes plus ou moins attaquées de l'Urédinée, que la carie n'est qu'un état morbide, qu'un dégénérescence de la globuline ou fécule du tissu cellulaire du périsperme du graine du blé.' A similar change, however, may also occur in the globules which naturally exist in the cells of the stem or in those of the leaves; so that, according to Turpin, 'L'Urédinée est une maladie qui attaque par place, les globules contenus dans les vésicules du tissu cellulaire des plantes, qui leur donne quelquefois plus de volume et

[1] It has been noticed that the leaves of many plants, prior to the appearance of fungi within them, have been remarkable for their 'almost unnatural green colour;' and, according to Mr. Cooke (loc. cit., p. 155), 'this phenomenon has been noticed in ears of corn, in which every grain was soon afterwards filled with spores of bunt.' This fact is one of much interest and importance, since it will be subsequently shown (in Chaps. XX and XXI) that the same extremely bright green colour is almost invariably to be observed amongst those portions of *Algæ*, or amongst *Euglenæ* and *Desmidiæ*, which are about to undergo a heterogenetic change; and in these latter cases every step of the process of transformation into new organisms may be watched by the microscopist.

[2] Loc. cit., p. 346.

toujours les couleurs blanche, jaune, aurore et brune, par lesquelles les mêmes globules passent dans les feuilles qui prennent toutes ces couleurs à l'automne. Ces globules ainsi vicié, peuvent ensuite, par contagion ou par inoculation altérer de la même manière ceux de la plante nouvelle.' Thus the mode of origin of the blight seemed so indubitable to Turpin, that he was led to suppose the products were mere pathological modifications of pre-existing structures, not possessing an independent life of their own. Several other celebrated botanists, moreover,—amongst whom we may name Fries, Endlicher, and Unger—were equally certain that these and many other Entophytes are derivable from morbid portions of the tissues of plants, although they recognised the fact of their developing into independent living organisms[1].

I first became convinced, from personal observation, that *Bacteria* and larger Fungus-germs may be encountered within the closed cells of living plants, about three years ago, during the examination of some specimens of sugar-cane in a sickly condition which were

[1] We quote the following note from M. Pouchet ('Nouvelles Expériences,' 1864, p. 117):—'Fries, qui classe ces plantes parmi les champignons, les décrit ainsi: *Entophyti vegetatio nulla. Sporidia ex anamorphosi telæ cellulosæ plantarum vivarum orta; sub epidermide enata et per hanc erumpentia.* Endlicher est encore plus explicite. Voici ce qu'il dit: *Sporidia varia e parenchymate morboso plantarum vivarum sub epidermide orta, hac rupta erumpentia, et varie sæpè mutata stipata.* Fries, Syst. iii. p. 501; Endlicher, Genera Plantarum, p. 16.' On this subject, see also Unger in 'Ann. des Sc. Nat.' vol. ii. n. s. p. 209.

brought under the notice of the Scientific Committee of the Royal Horticultural Society [1]. I found, on making thin sections of the central tissue even of young shoots, that many of the cells contained an abundance of *Bacteria*, and others a smaller number of *Torula*-like corpuscles, whilst some of the surrounding cells were quite free from either. I have since met with the same kind of thing when examining the central portions of decaying tubers and other fleshy parts of plants. Actual mycelial growths are, moreover, to be found in various situations, to which we might pretty confidently suppose that no external germs could ever have penetrated. They have been found, for instance, in the liquid juice taken from freshly broken cocoa-nuts [2], from the interior of walnuts and filberts [3], and from the central portions of stone-fruits, such as plums and peaches, whilst the surrounding and external fleshy portions were quite uninjured and unaffected. Speaking of *Botrytis infestans*, which he regards as 'the proximate cause of the potato murrain,' the Rev. M. J. Berkeley says [4]:—'The walls of the cavities of the carpels of

[1] An account of which is to be found in 'Journal of Royal Hortic. Soc.' vol. iii. 1872, p. 14.

[2] Dr. Sigerson writes:—'The author of this paper, having opened the dense shell of a cocoa-nut, and cut through its oily albumen, both perfectly intact to all seeming, found in the milk a web-like plant, a kind of Achlya.' ('Monthly Microscopical Journal,' Aug. 1870, p. 11.)

[3] The so-called *Tricothecium roseum*, for instance. I have seen a fungus-growth in the very centre of an otherwise healthy filbert.

[4] 'Introduction to Cryptogamic Botany,' 1857, p. 65.

tomatoes are often covered with the fungus, though there is no communication with the outward air; and a crop of the mould has been seen to grow in a few hours from the cut surface of a diseased potato even though the foliage itself had exhibited no trace of the parasite.' Multitudes of such facts might be referred to, and the facts themselves are, I believe, admitted by all. Dr. Lionel Beale, for instance, says:—'Lowly vegetable germs appear in closed cavities in the substance of dead animal and vegetable tissues. I have often seen them within vegetable cells in which not a pore could be discovered when the tissue was examined by the highest powers.' And again he says:—'I have detected them in the interior of the cells of animals, and in the very centre of cells with walls so thick and strong that it seems almost impossible that such soft bodies could have made their way through the surrounding medium [1].'

Again, nothing is easier for us than to discover such organisms within the very centre of the organs of dead animals, whenever the parts begin to exhibit signs of putrefaction. They are often met with in the centre of a mass of brain-tissue, for instance; and MM. Béchamp [2] and Estor have also observed that most active *Bacteria* in great abundance are always to be found in the midst of a portion of liver which has

[1] 'Disease-Germs,' 1870, p. 72. Dr. Beale's mode of accounting for these facts will be subsequently referred to.

[2] 'Compt. Rend.' t. lxvi.

been allowed to macerate in water for a day or two. When a section is made through such a mass, the cells in the very central portions are found to be swarming with moving particles and distinct *Bacteria*, although very few, if any, are to be seen in the water in which the portion of liver is immersed. M. Estor has, moreover, found that the cells of the liver in dogs, rabbits, mice, and various kinds of birds, even immediately after death, always contain a number of actively moving particles or mere granules (microzymæ), which both he and M. Béchamp believe have the power of developing into definitely formed *Bacteria*.

There can be little doubt that the granulations naturally existing in the cells of the liver and other organs, resemble those which increase under conditions of irritation, and which, under a more prolonged inflammatory stimulus are apt to undergo a fatty metamorphosis leading to the disintegration of the cell. But whilst these are the changes most prone to occur during life—especially in internal parts or organs—I fully believe (with other observers) that after death, or when death is close at hand, such particles may undergo an internal change fitting them for independent life, just as milk-globules are able to individualize themselves, and grow into embryo *Penicillia*. The union of two, three, or more of such granules in linear series has been watched by MM. Béchamp and Estor[1]. At first the granules form chaplet-like series, which gradually tend

[1] 'Compt. Rend.' 1868.

to become more cylindrical, so as to produce ordinary *Bacteria* and *Vibriones*. Similar phenomena have been testified to by Signors Crivelli and Maggi[1]. These observers watched the union of vitelline granules, and saw them gradually fuse into bodies in all respects resembling *Vibrio bacillus*, which in their turn gave rise to distinct *Leptothrix* filaments. Changes of a like nature were subsequently followed out by the same observers within epithelial cells taken from the back of the tongue of a diabetic patient. In this case the granulations of the epithelial cells, by their union in linear series, formed the rudiments of the future independent organisms. Moreover, Prof. Hughes Bennett has for several years asserted that such changes habitually take place, and has always laid much stress upon them, since it was in part owing to the occurrence of phenomena of this kind that he was induced to propound his 'Molecular Theory of Organization[2].'

On the other hand, we may watch all the stages by which epithelial cells in an apparently healthy condition become filled with the minutest granules which subsequently develop into well-formed *Bacteria*—just as particles similarly productive of *Bacteria* may be seen to appear within the substance of dying Amœbæ[3]. If healthy-looking epithelium cells from the inner side of the cheek are mounted and kept in a warm damp chamber, in the course of from twelve to twenty-four

[1] 'Rendiconti di Lombardo,' 1868. [2] See vol. i. p. 160.

[3] See p. 220, fig. 58, *i–m*.

hours a multitude of isolated and motionless specks make their appearance and speedily develop, within the substance of the cell, into well-formed *Bacteria*. This takes place when no notable amount of *Bacteria* exists in the surrounding fluid; and, indeed, from the mode of appearance, distribution, and development of the particles within the cell, it is obvious that, on the 'germ-theory,' we should have to believe that each epithelial cell which goes through this transformation is saturated with as many invisible germs of *Bacteria* as would correspond to the motionless and scattered organisms which are subsequently imbedded in its substance. In their earliest stage these units do not multiply; and before the contents of the cell become fluid, the relative positions of the individualizing units are maintained and may be well observed.

Thus, then, we have the possibility of independent organisms arising within unhealthy or dying cells, either by means of a heterogenetic modification of some already existing particles or globules, or by a process of new birth in the fluid or semi-fluid matter of the cell. By one or other of these modes, we believe that the various Fungi and other allied organisms, which are so frequently met with in the bodies of animals as well as of plants, are capable of arising *de novo*.

In the moister mucous membranes, *Bacteria*, *Vibriones*, and *Leptothrix* are most abundant; and, more rarely, larger Fungus-germs occur, which soon develop an

abundant mycelium, as where *Oïdium albicans* is produced in the affection commonly known by the name of 'thrush.' These various organisms exist abundantly enough in almost all the mucous membranes of the body, more especially when there is some unhealthy mode of action going on in the part; and their prevalence in these situations is far more dependent upon the presence or absence of such conditions than upon the degree of exposure of the part to the possible contaminating influence of germs derived from without. Some of those which are least exposed are most prone to throw off the organisms already mentioned, as well as Monads and other more animalized forms[1].

Fungus-germs and rudimentary mycelia are also frequently met with upon, and in, the superficial layers of the skin of man and of the lower animals, where they represent the best known characteristics of certain familiar diseases.

Here again, as in the case of the mucous membranes and of the general parasitic diseases, there is the possibility that such growths may be occasioned by actual contact with some disseminated and all-pervading Fungus-germs. We know, indeed, that these parasitic diseases are contagious; that persons free from such maladies may become affected, provided the infecting

[1] See Dr. Gros on Vaginal Animalcules, in 'Bull. de la Soc. de Nat. de Moscou,' 1845, p. 426; and Mr. T. R. Lewis in his previously quoted 'Report,' as to forms which may be met with in the intestinal canal.

germs fall upon suitable situations which are in a condition favourable to their growth. Even here, however, the conjoint influence of predisposing and exciting causes of disease must come into play. So that the question is whether, in certain cases, the 'predisposing' causes may not be sufficiently potent to generate the disease, without the aid of any 'exciting' cause in the form of pre-existing Fungus-germs. Much evidence of a general character, in addition to the many facts and observations already alluded to, tends to favour this view—more especially in the face of the insuperable difficulties which beset those who are exclusive advocates of a 'germ-theory.'

In reference to the above-mentioned skin-diseases in the human subject, Dr. Tilbury Fox[1] calls attention to the fact of the extreme frequency with which the hair-follicles are the seats of the first manifestation of the morbid product; and says that, for the most part, 'the very first spot at which any perceptible fungus can be detected is a little way inside the follicles, near the opening of the sebaceous glands.' Thence the fungus-growth extends in various directions—into the follicle itself, into and upon the hair, and into the immediately adjacent portions of skin. In other cases, as in 'chloasma,' it is the substance of the epithelial cells over the chest or abdomen which is the seat of the fungus spores and filaments.

Certain altered states of secretion from the hair-

[1] 'Skin-Diseases of Parasitic Origin,' 1863, p. 43.

follicles, under the influence of derangement or peculiarity of constitution, seem to be most favourable to the occurrence of nearly all the cutaneous diseases which are attended by the presence of vegetal parasites. In the case of chloasma especially, we constantly see that its presence is associated with the occurrence of certain favouring conditions. Thus it is met with most frequently in persons of delicate health, who wear flannel next the skin and do not have recourse to sufficiently frequent ablutions. The increased moisture from retained secretion, and the increased heat, seem sufficient in certain persons to engender this disease of the skin. It is one of those affections in which we have the least reason for supposing contagion necessary, and in which we least frequently find any evidence of it. In epithelial cells affected in this manner we frequently may see a number of highly refractive particles of various sizes, having a close resemblance to fatty globules and granules, though they are probably composed of a combination of fatty and albuminoid matter[1]. These globules and particles are often indistinguishable from precisely similar-looking bodies,

[1] Such particles are always most abundant in the white secretion with which some of the sebaceous follicles are apt to become filled. If a specimen of it is mounted in a drop of distilled water and flattened by a covering glass, and then enclosed in a wide-mouthed stoppered bottle containing a little water, so as to prevent evaporation of the drop of water in which the specimen is mounted, fungus filaments will often be found to develop from all parts of the sebaceous mass in the course of two or three days.

which, by their subsequent growth, prove to be germs of fungi. It seems to me, therefore, highly probable that, under certain conditions, it is these oleo-albuminoid globules, already in existence, which in consequence of some internal changes are impelled to develop, like ordinary milk-globules, into Fungus-germs[1].

This view derives additional support from the fact that, on other occasions, Fungus-growths of various kinds have been found, in different kinds of animals, in internal cavities of the body which are wholly closed—just as they have also been found within similar cavities occurring in the fruit of many plants. The Rev. M. J. Berkeley says[2]:—'The strongest case I

[1] In accordance with the more generally received hypothesis, however, we should be required to believe that the fungi in the different diseases were specifically distinct, and that spores of the different varieties were so uniformly diffused through the atmosphere as to be always ready to infect any suitable nidus. The difficulties of this hypothesis were felt by the Rev. M. J. Berkeley, who accordingly wrote in his 'Introduction to Cryptogamic Botany' the following passage:—'It is true that in many cases the fungi may be of very common kinds, or under disguised forms; but this is what might readily be supposed, for it is very rarely the case that such peculiar matrices as the human skin or mucous membrane should nourish fungi absolutely peculiar to themselves. It is in such cases far more easy to believe that the common *Penicillia* or *Aspergilli*, which are notoriously indifferent about their matrix, provided the proper chemical conditions be satisfied, are the real antagonists' (p. 238). This view was afterwards ably taken up by Dr. John Lowe and Dr. Tilbury Fox, who endeavoured, with considerable success, to demonstrate the convertibility of the several forms of fungi met with in skin-diseases, and their relationship to the forms above mentioned. This was decidedly a step in the right direction.

[2] Loc. cit., p. 260.

have met with is the development of a yellow mould within the cerebral cavity of golden pheasants, which soon proved fatal'; whilst Dr. Murie[1] has found fungus-growths within the abdomino-pleural membrane of a Kittiwake gull, of a great white-crested cockatoo, and of a rough-legged buzzard. Well developed fungi have been found, moreover, within the uninjured eggs of birds and serpents by Cantoni[2], Rayer, Robin[3], and others; and this not merely just within the shell and its living membrane, but growing from the surface of the yolk itself.

Certain insects, moreover, are not unfrequently the seats of remarkable parasitic growths, which, although they may after a time manifest themselves externally, either take origin in, or at all events start in their development from, some internal portion of the body, and are unknown to exist in any other situations. This is the case, for instance, with the strange *Sphærea Robertsi* which grows from a caterpillar (*Hepialus virescens*) in New Zealand; with *Sphærea entomorhiza* which appears in pairs from the middle line of the back, at the junction of the thorax and abdomen, in many of the West Indian beetles; and with many other anomalous growths which have from time to time been met with upon members of the insect world.

[1] See abstract of his communication in 'Report of British Association,' 1871.

[2] 'Rendiconti di Lombardo,' Nov. 1867.

[3] See his 'Végétaux Parasites,' Plate ii. Figs. 5 and 6, and Plate iv. Fig. 9.

Nay, fungi have been found even within the human eye. Helmarecht records the case of a man who, having previously suffered from an inflammatory affection of the eyes, became rather suddenly troubled with images of a constant character floating before the left eye. On examination, a morbid product was found suspended in the posterior chamber just in front of the lens. Soon afterwards the lower part of the cornea was punctured, with the hope that the separated foreign body might be washed out with the evacuating aqueous humour. This actually occurred, and it was then found to be a minute confervoid-looking growth, bearing series of spores arranged in chaplet-like fashion [1]. We will merely mention the fact of the existence in India of a terrible disease known by the name of 'Madura foot,' in which a fungus, closely allied to some Mucors, seems to extend outwards from the bones and ligaments, growing destructively through all the tissues of the part. In exhibiting a specimen at a recent meeting of the Pathological Society, Mr. Jabez Hogg expressed his belief that this fungus takes origin from some altered tissue-elements

[1] See Robin's 'Végétaux Parasites,' 1853, p. 370. He adds :—'Après l'opération le malade se trouva bien, et continua ses occupations sans gêne.' Robin also mentions a case (p. 364) in which a larger though somewhat similar growth was met with by M. Gubler growing beneath the skin of the back of the hand of a young man suffering from a gunshot wound. Several white bleb-like elevations of the skin were produced, which were found to have been occasioned by the growth in question.

in the foot itself, rather than from externally derived germs.

The occurrence of such growths within closed cavities not exposed to the incidence of external germs, and their not more frequent presence in the ramified air-chambers of birds (where germs might so well lodge and grow), is just as much against the supposition of their external derivation, as the comparatively rare presence of the growths in these cavities militate against their being developments of some of the many minute and invisible germs with which the tissues of higher organisms are believed, by a few persons, to be permeated. This latter view is only countenanced by the occurrence of facts which are thought by others to be equally capable of receiving a totally different explanation.

During the last fifteen or twenty years, another most important disease, 'Pébrine,' has prevailed amongst the silk-worms of France, which, while of the utmost consequence commercially, has seemingly been due to the ravages produced by certain extremely minute vesicular parasites usually known as *Psorospermiæ*—organisms concerning whose real nature and affinities systematists are not agreed. Some place them amongst the equally obscure group of Gregarinidæ; whilst others, such as Robin and Balbiani, consider them to be more allied to Diatoms or aberrant forms of Algæ. Organisms of a similar nature, though rather more complex in structure,

were first discovered about thirty years ago by J. Müller, pervading the spleen, kidney, and almost all the organs and tissues (with the exception of the great nerve-centres and the muscles of the trunk) of certain fresh-water fishes. They often follow the line of the blood-vessels, and are occasionally concentrated in such enormous numbers as to form small whitish or yellowish masses visible to the naked eye—although individually the organisms are only a very little larger than the blood-corpuscles of the fish. Subsequent investigations have revealed the fact that these rudimentary organisms are to be met with habitually in the tissues of higher as well as of lower organisms, where, for the most part, they seem to exist without the slightest detriment to the creatures which contain them. Six years ago, when the cattle-plague raged with great virulence in this country, some observers thought for a time that they had detected its cause, when they found myriads of these minute corpuscles imbedded in the flesh of animals which had succumbed to the disease. But a distinguished helminthologist, Dr. Cobbold[1], soon made known the fact that such organisms might be met with most abundantly in the healthiest mutton and beef; and that they were always to be found in astonishing numbers in the substance of the heart of sheep, oxen, and other animals. They had, in fact, nothing whatever to do with the cause of the cattle-plague. Similar parasites have been found, on several occasions, in

1 'Lancet,' 1866, vol. i. p. 88.

various parts of the human body; and such living units seem to exist habitually in almost all varieties of lower organisms. They have been encountered, for instance, in the majority of insects and in many other classes of Articulata; whilst Balbiani[1] has also discovered them amongst the Arachnida and in many fresh-water Entomostraca. In the latter animals, and also in some serpents, as Vlacovitch ascertained, the corpuscles are of the same simple characters as those which exist in certain diseased silk-worms; although in fishes and other organisms[2] the corpuscles are often more complex. They are then composed of a mass of granular protoplasm enclosed within a resistent bivalved envelope, having a projecting rim at the junction of the valves, and two internal ovoidal projections either at one or at both extremities. They multiply with great rapidity, for some of the corpuscles are capable of growing to fifty or one hundred times their original size, so as to constitute generative bodies, which, by a process of segmentation, resolve themselves into a new progeny of Psorosperms—just as *pseudo-navicellæ* are produced from Gregarinæ.

These organisms being, therefore, so extremely common, and, for the most part, so innocuous to the animals which they frequent, it is somewhat surprising to find them apparently producing a fatal epidemic disease amongst silk-worms. It is, however, absolutely

[1] 'Journ. de l'anatom. et de physiolog.' 1866, p. 599.

[2] Including some insects, such as *Pyralis*, according to Balbiani.

certain that in this affection—named 'pébrine' on account of the black spots which are produced on the skin—Psorosperms of the simplest description abound in almost every organ and tissue of the affected worms

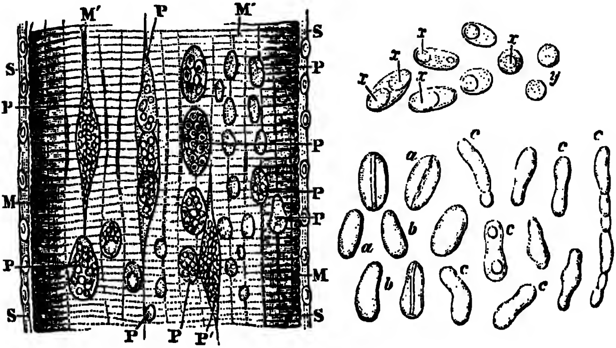


FIG. 74.

Psorosperms and their Mode of Development. (Balbiani.)

a, a. Psorosperms from the Silk-worm.
b, b, and *c, c.* Rare and occasional varieties of these.
x, y. Psorosperms in an earlier stage of development, which are found intermixed with the more perfect forms. (× 1700.)
M, S. Portion of the intestine of a small Caterpillar (*Gastropacba*), showing masses of Psorosperms in different stages of development beneath the serous coat. At first, mere homogeneous masses of matter (P, P) appear, in the midst of which Psorosperms are developed after the manner of nuclei. (× 250.)

or moths. Whether they are causes of the disease, however, or are mere concomitant products, really occasioned by a previous blood-change (and therefore comparable with the fungi in muscardine), it is impossible for us to say: though it is certain, from the

researches of Pasteur, Balbiani, and others, that the disease is eminently contagious[1]. Pébrine, moreover, unlike muscardine, may be transmitted hereditarily. In these cases, the affected eggs, according to Balbiani, always yield a slightly acid reaction when crushed upon litmus-paper, whilst healthy eggs are neutral. And although Psorosperms are not visible at first in such infected eggs, they gradually develop from particles which are originally quite indistinguishable from the ordinary granules of the ovum. These facts are, therefore, quite consistent with the possibility of the hereditary transmission of a mere morbid quality to the ovum, whose presence, whilst already attested by the acid reaction above referred to, may soon display itself still further by the occurrence of a new evolution of Psorosperms.

On this theory the disease might be contagious,

[1] According to M. Robin, Psorosperms are frequently developed in the midst of an amorphous and homogeneous 'masse sarcodique génératrice,' which insinuates itself or lies between the elements of the tissues in all directions, so that they are often seen imbedded in an amorphous substance. Further researches concerning the mode of origin of this material are much to be desired. Professor Leuckart indeed says:—'It appears to me in no way made out whether the Psorospermiæ are to be considered as the result of a special animal development; whether they, like pseudo-navicellæ, are the nuclei of gregariniform productions, or whether they are the final *products of pathological metamorphosis.*' And again, in his 'Untersuchungen über Trichina Spiralis,' 1860, in speaking of the enteritis which is occasionally induced by these Nematoid parasites, he says that 'croupy masses,' which are sometimes thrown off in flakes, may at other times resolve themselves into pus-corpuscles, or in dogs may be 'converted into Psorospermiæ.' (See Dr. Cobbold's 'Entozoa,' p. 344.)

owing to the corpuscles acting after the manner of mere dead ferments. They might incite certain changes in the fluids with which they came into contact; and these changes would culminate in the re-evolution of new and independent organisms in the blood and tissues generally, or in some parts of them—just as the poisonous organic particle from the skin of a small-pox patient incites spreading changes in the body of a person with whom it comes into contact—changes which terminate in the re-evolution of a similar poison [1].

The opposite notion—viz. that the communicated disease is caused by the direct multiplication of the organisms which act as contagious agents—is beset with difficulties. Seeing how exceedingly common Psorosperms are in the bodies of all kinds of animals even in their natural or healthy condition, why should they not multiply to an undue extent, and produce disease in them? Such organisms are probably swallowed by most of us during each meal in which meat is taken, and yet no harm ensues—they are probably just as nutritious as the meat in which they are contained. Speaking of their almost universal prevalence, Dr. Cobbold says:—'Altogether, at two meals, I could not have swallowed less than eighteen thousand of these Psorospermiæ;' and he adds that those who consume 'beef, mutton, and pork, eat these bodies every day, but take

[1] I have elsewhere (*Appendix* E, p. cxv) expressed the belief that the germs of existing morbid growths also operate in this manner when they are the causes of 'secondary growths.'

no harm.' Again, silk-worms probably contained Psorospermiæ, and swallowed them at times with their food, long before the outbreak of pébrine, and yet they were not affected by a fatal epidemic disease. At last, however, a particular change or morbid condition of their tissues seems to have been initiated, in which Psorospermiæ are especially prone to be developed—these being, moreover, characterized molecularly by some peculiar properties, so that they are capable of initiating similar primary changes in other silk-worms [1].

Looking, then, broadly at the various facts which have been detailed in this chapter, how are they to be explained by those who refuse to believe in Archebiosis and Heterogenesis?

The only attempt at an answer which can be adduced may be given in the words of Dr. Lionel Beale, who says [2]:—'The higher life is, I think, interpenetrated, as it were, by the lowest life. Probably there is not a tissue in which these germs are not; nor is the blood of man free from them. They are found not only in the interstices of tissues, but they invade the

[1] The rational conclusion concerning pébrine, therefore, comes to be very similar to that which we are compelled to adopt concerning the mode of origin and propagation of 'flacherie'—another disease of silk-worms, produced by feeding upon unhealthy or fermenting mulberry leaves, and characterized by the presence of minute *Torula*-like corpuscles throughout the body. ('Brit. Med. Journal,' March 9, 1872, p. 259.)

[2] 'Disease-Germs,' 1870, p. 64.

elementary parts themselves.' This represents, in general terms, the doctrine or mere assumption which is made by many at the present day, who cannot otherwise account for the numerous phenomena to which we have just been referring. It is, however, in direct opposition to the experiments of Dr. Burdon Sanderson, and is antagonistic to the theory and practice of those who believe in antiseptic surgery. It is an hypothesis of Panspermism of a kind very similar to that which was started by Bonnet more than a century ago. But this old view is now sought to be maintained under the most adverse circumstances. Originally supported by no independent observations, and based only upon the occurrence of phenomena which could be otherwise explained, it was thought to derive support from those views concerning development which were at the time advocated by Haller and Bonnet. But these old theories as to the 'pre-existence of germs' of all organs and tissues in the ovum itself, have faded away like a cloud before the light-giving doctrines of Harvey, Von Baer, and the modern evolutionists. With the disappearance of the old theories of development, vanished all that could ever have been said to give countenance to this doctrine of Panspermism — nay more, it now stands in even a more sorry plight, since it is condemned and undermined by many of the recent and most positive testimonies of scientific workers[1].

[1] See pp. 6 and 7.

Quite independently of all this, however, it will be found, as we have already hinted, that an exclusive doctrine of contagion is singularly unsupported, and inadequate to account for the spread and perpetuation even of undoubtedly parasitic diseases. In this respect, the history of these diseases is essentially similar to the history of those non-parasitic but contagious febrile diseases which frequently prove so disastrous to the human race. A comparison of the respective phenomena which characterize them, tends to show that the parasitic and non-parasitic affections are more intimately allied than might be supposed. Both sets of diseases are due to the occurrence of spreading changes of a general character throughout the body, leading to grave alterations in the constitution of its principal fluids[1]. These changes in the ordinary exanthemata are not such as to lead to a birth of independent organisms within the blood and tissues[2]; whilst in other affections they do entail the birth of a multitudinous progeny of independent units. And partly because the changes which lead to this birth are probably more complete, and more antagonistic to the ordinary vital processes—though even more on account of the enormous powers of reproduction possessed by such new-born independent units—the higher organisms which are thus affected are nearly sure to die. This almost

[1] See *Appendix* E, pp. cxvii and cxlvii.

[2] See 'Trans. of Patholog. Soc.' 1869, p. 426.

invariable fatality of the general parasitic disease is precisely what might have been anticipated.

It would, therefore, follow from these views, (1) that the development of the parasites in the latter affections is altogether secondary, in order of time, to the blood-changes by which they are produced; and, as I have already hinted, it would seem to indicate (2) that even in cases of the spread of such diseases by contagion, the contagious element, whether living or not living, operates by its power of initiating certain molecular changes—which, gradually extending throughout the body, may or may not in their turn cause the evolution of new organisms. That is to say, we have, at first, always to do with a mere contact-action, and even in the case of general parasitic diseases, the *direct* multiplication of the infecting agent itself is only an unimportant accessory process as compared with the spreading changes initiated by its contact-action.

Already-existing evidence is thoroughly harmonious with such views.

In the first place, it is admitted even by those who are pure contagionists, that a blood-change is the primary and necessary initiator of one of these diseases. Thus, M. Robin says:—'Les circonstances qui paraissent favorable au développement de la Muscardine sont celles qui ont pour premier résultat une altération des humeurs ou des organes de l'animal vivant, et c'est à la suite de certe altération que le parasite se développe. . . Le développement du *Botrytis* est donc bien plutôt con-

sécutif au modifications des humeurs que cause de celles-ci.' And then he continues:—'Les faits d'introduction artificielle des spores ne sont pas en contradiction avec ce qui précède, car la piquer pratiquée pour introduire le mycélium et les spores est suffisante pour déterminer ces modifications des humeurs, d'abord localement, puis peu à peu dans toute l'économie[1].' We know, moreover, that the blood invariably yields an acid reaction by the time that the first organisms appear in it.

The second point is best established with reference to the phenomena of that fatal epizootic disease amongst cattle, which is commonly known by the name of the 'blood' or 'sang de rate.' The researches of M. Davaine in connection with this subject are of the highest value. The malady is characterized by the presence of multitudes of Vibrio-like organisms in the blood, which, however, differ from the Vibriones of ordinary putrefactions. Whilst this affection is always capable of being reproduced in a previously healthy animal by the inoculation of some of the fresh blood of an animal which has recently died of the disease, the blood of such

[1] 'Végétaux Parasites,' p. 585. Mere contact of the spores with the skin, when the animals are at all predisposed, seems to suffice. General blood-changes are soon induced. Thus, M. Guérin-Méneville rubbed some *Botrytis* against a chrysalis; and, on examining the blood of the same animal on its first appearance as an imago, he found multitudes of the characteristic elongated bodies in its blood; and on its death, several days afterwards, these were observed to have become ramified and much increased in size (p. 580).

an animal loses its powers whenever it becomes putrid [1]. This has been established by multitudes of experiments. A drop of the diseased blood inserted beneath the skin of rabbits was found to be always sufficient to reproduce the disease; whilst a drop of ordinary blood, after it had become putrefied and swarming with *Bacteria*, when similarly introduced, generally produced no effect. Again, rabbits which had been fed upon the fresh organs of some animals that had recently died of the 'blood,' almost invariably became affected by the same disease, and soon showed myriads of the characteristic organisms in their blood. Whilst of other animals which were made to swallow similar quantities of liver, after it had become quite fœtid (and therefore swarming with *Bacteria*), only one out of eight died; and even that one, which was found to have suffered from an inflamed lung, did not reveal any trace of organisms in its blood.

These experiments seem only explicable on the assumption that, in the cases where the 'blood' was communicated to other animals by inoculation (either subcutaneously or by the stomach), the disease was communicated not so much by the direct multiplication of the stock of inoculated organisms and their spread throughout the body, as because some of the inoculated

[1] M. Davaine says:—'Dans les grandes chaleurs de l'été lorsque le thermomètre marquait de 28 à 32 degrés centigrades, j'ai vu disparaître la faculté dont il s'agit en quarante ou cinquante heures, une fois en trente-cinque heures.' ('Compt. Rend.' 1864.)

matter (either fluids or organisms) had the power of setting up certain changes of a spreading character, which soon sufficed to produce a condition of blood similar to that usually preceding the development of organisms in this disease. If the organisms acted *quâ* organisms, and not as ferments or producers of spreading chemical changes in the fluids of the body, then we should necessarily expect that other more or less similar organisms would also be capable of multiplying themselves, and of producing general parasitic diseases. This, however, is notoriously not the case. Fermenting and semi-putrid articles of food, teeming with lower organisms, are constantly eaten with impunity by the lower animals, and are in many instances sought after by man—nay, such articles of diet are occasionally administered with the view of curing rather than with the prospect of causing disease[1]. We can only conclude, therefore, that in 'the blood,' in 'flacherie,' and other similar affections, the contagious element acts as a mere dead ferment might do in inciting blood-changes; and that the organisms which are subsequently found in the infected animal are, for the most part, the products of a new birth which has taken place in the altered fluids[2].

[1] Take the case of 'Kousso,' for instance, which is lauded by some as an excellent remedy for Phthisis. See also *Appendix* E, p. cxxiv.

[2] Other instances of a similar nature are known. Thus, it has been ascertained by M. Vulpian ('Archives de Physiologie,' vol. i. 1868), that the insertion of a small portion of cyclamen-root beneath the skin of frogs produces local irritation and a putrefactive process, which, after

Again, all that has been said with reference to these general diseases is abundantly borne out by what we know of local parasitic affections. Here, too, the general or predisposing causes represented by the state of the parts themselves, seem to be just as important as the action of the special contagion—indeed, in multitudes of cases, the action of the special contagion is assumed rather than proved to exist. Just as muscardine can, apparently, be 'spontaneously' engendered in almost any caterpillars which are placed under certain conditions, so does the Fungus-growth which is characteristic of 'thrush' manifest itself on the tongue and adjacent parts of children under certain conditions, or even on that of adults in the last stages of lingering illnesses. The thoroughly healthy silk-worm is, moreover, almost proof against contagion, just as the tho-

a few days, is followed by signs of languor and great debility in the animals operated upon, and is succeeded by a speedy death. Both during life and after death, such animals possess myriads of *Bacteria* in their blood. And yet the disease is not produced by the mere presence of *Bacteria* in the local seat of irritation. I have several times injected one or two drops of a fluid swarming with *Bacteria* beneath the skin of frogs, without producing any such effects, or leading to the subsequent presence of *Bacteria* in the blood. The cyclamen doubtless sets up peculiar local changes, which are capable of spreading, so as to produce a general disease in which the blood as well as other parts of the body are affected; and so far there is an agreement between such a process and pyæmia in the human subject after wounds or operations. In the latter case, however, the blood-changes are not such as to lead to the evolution of *Bacteria* during the life of the individual; whilst, in the case of frogs inoculated with cyclamen-root, the changes in the blood do lead to the evolution of organisms.

roughly healthy child or man is proof against the attacks of *Oïdium albicans*. When from lowered health, however, the blood of the silk-worm tends to acquire an acid reaction, or the mucous membranes of the infant or of the man assume such a condition, then the respective organisms may appear, and general or local morbid conditions may be established[1].

Again, the experiments of Dr. Spring[2], in many attempts to produce a direct inoculation of hens' eggs, were very unsuccessful, even though the inoculation was made with portions of a fungus which had been produced in an uninjured egg. Very rarely, indeed, did a growth start from the seat of inoculation, though frequently an entirely different form sprang up at the opposite extremity of the egg, in a region which was apparently quite beyond the reach of contamination. And, again, such facts are rendered all the more significant by reason of the interesting researches of Harless[3], showing that fungi may be made to appear, almost at will, in eggs which are exposed for a few days in the chamber of an incubator saturated with

[1] How important such a modification may be, in enabling evolutional changes to occur in certain fluids, is to be surmised, as we have previously hinted, from the often quoted experiments of Dutrochet, who found that white of egg diluted with distilled water remained for more than a year without becoming covered by mould; whilst, by rendering it slightly acid, a crop of these organisms became developed in less than eight days.

[2] 'Bullet. de l'Acad. Roy. de Belgique,' 1852, t. xix. p. 573; or abstract in Robin's 'Végétaux Parasites,' pp. 545–54.

[3] Quoted by Robin, loc. cit., p. 559.

moisture and maintained at a temperature of 100–104° F. Under these conditions, the customary exhalations and evaporations of fluid cannot take place from the egg, so that the embryo dies whilst lower organisms appear[1].

The facts narrated in this communication, and multitudes of others of a similar nature that might have been quoted, constitute a body of evidence thoroughly harmonious with the results of my previous observations and experiments. And yet this interpretation will doubtless be, for a time, rejected. Many of those who have wholly cast on one side the old developmental theories of Haller and Bonnet, still confidently pin their faith to a derivative doctrine. And when we find this doctrine of Panspermism advocated not only by professed vitalists, but by some leading evolutionists, the inconsistency is notably increased. They have never attempted to explain why those natural life-evolving laws, whose original existence they postulate, should have ceased to operate in the present day. They assume the omnipresence of germs; they assume that such hypothetical germs can exist for an indefinite period in a latent state; that they can resist degrees of heat which are fatal to all known germs; and that

[1] Almost similar facts may be cited concerning plants. Attempts at direct inoculation with parasitic fungi, as Dr. Barry found, are far from being so successful as might have been expected. See some remarks on this subject in 'Brit. Med. Journal,' April 20, 1872, p. 419.

they possess powers of penetration such as known germs do not possess—they will make all these assumptions upon assumptions in order not to believe what is in accordance with the general teachings of science and with their own doctrines in particular, what is testified by experiment, and also that which almost every unbiassed microscopist may learn by direct observation.

CHAPTER XX.

HETEROGENESIS IN LOWER ORGANISMS.

Higher and Lower Organisms. Their Modes of Death. Matter of Aquatic Lower Organisms most prone to undergo Heterogenetic Changes. Dr. Pringsheim's Observations on Algæ. Dr. Braxton Hicks on production of Amœbæ in Moss-radicles. Development of Amœbæ into Ciliata. Mr. Carter's observations on Nitella. Production of Monads. M. Nicolet on Mode of Origin of Amœbæ and Actinophrys. Formation of Trichomonas. Its Conversion into other Forms. Mr. Carter on Development of Actinophrys in Spirogyra. Origin of Monads and Amœbæ. Development of Pythium in 'resting-spores' of Algæ and in Rotifers. Appearance of Astasiæ within Cells of Spirogyra. Other Heterogenetic Changes in Algæ and Desmids. Author's Observations on Vaucheria. Mode of Origin of Amœbæ and Actinophrys. Heterogenetic Changes in Nitella. Embryonal Spheres. Their various Transformations. Origin of complex egg-like Bodies. Transformations of Chlorophyll Corpuscles. Analogous changes previously described. Dr. Braxton Hicks on the Heterogenetic Origin of Glœocapsa. Dr. Gros on Origin of Diatoms and Desmids. Confirmation of his Views. Origin of Desmids from Chlorophyll Corpuscles. Other Modes of Development of Desmids and Diatoms. Modes of Origin of Euglenæ. Interesting Nature of these Organisms. Their various Transformations. Reasons why Algæ and allied Organisms give birth to such varied Products. Bearing of Facts recorded upon Zoological and Botanical Classifications.

WE pass now to a consideration of the heterogenetic processes which occur in much lower forms of life. And it should be borne in mind when studying the changes that take place in the proto-

plasmic tissue of various aquatic plants and animals of a low grade of organization, that such living things exist and die under the very conditions which might have been imagined to be most favourable for the occurrence of transformations in the matter of which they consist. The living matter of these organisms exists in a semifluid state, and is exposed, in comparatively small masses, to the influence of various physical forces acting through the fluid medium in which they are immersed. When units of living matter exist as constituents of one of the higher organisms (in which the actions of all the different parts of the body are nicely balanced and subordinated), their individual actions in different parts induce, and are necessary for, the maintenance and increase of the whole as a whole. But in one of the filamentous fresh-water Algæ we meet with such a mere aggregate of organic individualities—of parts potentially separate—that when the conditions of the medium in which it lives become in any way unsuitable, its molecularly-mobile and impressible tissue soon begins to feel the influence of such change: modes of action and interaction are set up between the organism and its medium which speedily become quite incompatible with its further existence as an Alga. The molecular changes induced in its interior are of such a kind that the several parts of each of the aggregates of which it is composed first cease to work in harmony, and afterwards may be still more modified, so as to make it

utterly impossible that the matter which undergoes such changes should continue to exist under the form of an Alga. But, although changes of this kind have been set up—although altered modes of molecular activity have been induced in response to new external pulses or movements—surely there is no reason for supposing that the matter of which such an organism is composed must necessarily 'die,' simply because it can no longer exist under its old form.

As we have already endeavoured to show [1], the death of an animal, from a physical point of view, merely means the cessation of those particular actions and reactions which were accustomed to go on in its body, and which are essential to its existence as such an organism. Its organic structure had been produced under their influence, and its form could not be maintained without their continuance. Its several tissues had been built up, and were what they were, solely on account of their relations to one another and of the mutual performance of certain functions more or less necessary for the well-being of the organism as a whole. Let the throbbings of the heart cease for a while, and the highly-organized vertebrate animal dies. Pabulum is no longer supplied to its elementary parts, and, as minute by minute passes by, changes take place in its most sensitive tissues, which render a renewal of the life-giving conditions more and more difficult. At last it becomes impossible again to set

[1] See vol. i. pp. 108–112.

on the all-important blood current. The organism which but a minute before was living, is now dead, —dead that is as an organism, as a marvellous mechanism, capable, perhaps, of performing the highest functions of humanity; but the matter is still there, and in a 'living' state, even though the organism as a whole is dead and spiritless. The nerve will still for a time transmit a stimulus, the muscle will still contract—the cilia of various mucous membranes still lash on, as though no change had taken place. But in descending step by step from highest animal to lowest animal, and—though far less obviously—from highest plant to lowest plant, we find the organism less and less complex, and, at the same time, we find the nexus less obvious which binds their several parts into one whole. And at last we meet with organisms altogether simple, or else made up of mere repetitions of similar parts, so that the individualizing nexus is reduced to its lowest terms, and the separate parts live and die more or less independently.

The great difference existing between a layer of ciliated epithelium on the human trachea [1], and an expansion of more or less similar cellular compartments constituting one of the ulva-like Algæ, is due to the fact that the former has been produced in a situation and under the influence of a set of conditions so much more complex than those which have given birth to the Alga, that its structure is, to a corresponding extent,

[1] See vol. i. p. 145, Fig. 4.

more specialized and more sensitive. The epithelial layer is dependent for its life upon the continuance of a set of conditions which are only possible so long as it constitutes an integral part of a living organism. Soon after the death of the human being of which it formed part, the interference with, and ultimate arrest of, the conditions under which the epithelial layer was fitted to live, entails a gradual arrest of those molecular actions which made up the life of its several parts. The Alga, however, being an independent organism, is amenable only to more general conditions, and, during its periods of molecular re-arrangement, it continues to exist in the fluid medium in which it has been born.

But, for the morphological units of each alike—for the epithelial cell, and for the algoid cell or compartment—we are warranted in claiming a kind of organization. They, as simplest morphological units, are themselves organisms compounded of living molecules. And, just as we have seen that the death of an organism, as a whole, does not entail the death of its several parts, unless it happens that such death of the whole organism brings about, as a necessary consequence, a cessation of those conditions under which alone the several parts are fitted to exist; so does it seem reasonable for us to imagine that the conditions under which certain of these simpler organisms exist may be such as to enable their life to persist under other forms, when a continuance of the form previously in existence becomes no longer possible. The complex

molecules of such simple organisms may rearrange themselves, and, under the influence of disturbing conditions, may fall into new, simple and compound modes of aggregation. Thus new centres of attraction may arise, new current modes of activity may be initiated, and new organic forms may result. So that the constituent elements of the previous organism may be still present, living and mobile, although differently combined, and variously reacting upon the ethereal pulses of heat and light to which they are subjected. In some such manner must we explain the occurrence of the various metamorphoses of living matter of which we are about to speak.

Nearly twenty years ago, Professor Pringsheim[1] called attention to the production of what he believed to be a peculiar kind of propagative spore, in the cells of young filaments of *Spirogyra jugalis*, and also in certain conjugated cells of the same Alga before, and indeed instead of, the production of the ordinary resting spores. He says he frequently found, in conjugated filaments, 'that the contents of one or more pairs of conjugated cells were not transformed into the well-known large spore.' They, however, 'became transformed into a number of little cells of regular, definite, and unchangeable form,' whose constant occurrence led Dr. Pringsheim to conjecture that they were 'more than mere pseudo-forms of decaying cell-

[1] 'Ann. and Mag. of Nat. Hist.' 1853, vol. xi.

contents.' He first obtained an insight into these structures by observation of their production in the cells of young *Spirogyræ*, which he had himself developed from large spores. He says:—'In the cells of these young *Spirogyræ* the existing spiral bands are often broken up, and from their substance are formed, in a manner unknown to me, little cells in which a membrane can be clearly detected surrounding green contents. I call these cells, spore-mother-cells. They soon increase in size, their membrane separating itself from the contents and expanding into a large hollow vesicle. The contents at the same time acquire a yellowish or yellow-brown colour, and separate into a central, denser, yellow-brown nucleus, and a finely-granular mucilage, which surrounds the nucleus and does not entirely fill the space between it and the membrane. This finely-granular mucilage then becomes balled together, in the space between the yellow nucleus and the surrounding membrane, into a single large corpuscle exhibiting a sharply-defined outline, and appearing as a transparent vesicle with finely-granular contents. The new cell thus formed pushes the brown body, as the figures show[1], out of its central position, against the wall of the parent cell or the spore-mother-cell. The pressure of these two bodies causes the rupture of the membrane of the spore-mother-cell; the *transparent* cell emerges and moves about independently and freely in the filament cell in the manner of the zoospores.' They moved

[1] Dr. Pringsheim's paper is illustrated by several figures.

about by means of cilia which sometimes existed singly and sometimes in the form of a circlet. Although Dr. Pringsheim was disposed to look upon these bodies as anomalous propagative spores belonging to the plant itself, and destined to reproduce it, he came to such a view principally because of his certainty as to their mode of origin. He says:—'That they are foreign structures, not belonging to the *Spirogyræ*, would be an altogether inadmissible hypothesis, *since they are formed in the interior of the closed filament cells of the* Spirogyræ, *directly from their contents*.' Dr. Pringsheim had seen bodies originating after a similar fashion within the resting spore of *Œdogonium vesicatum*, and within the filament cells of *Cladophora fracta* and of *Nitella syncarpa*, and as he had not seen the further developmental modifications of these newly-produced organisms, the view which he adopted seemed the only one open to him at the time. Now, however, there cannot be much doubt as to the relationship which exists between these bodies and the organisms whose evolution Mr. H. J. Carter has watched, within the filament cells of weeds belonging to the same genera, and which Dr. Braxton Hicks[1] has seen originating within the elongated alga-like cells of which moss-radicles are composed.

In order to procure such alga-like moss-roots in a suitable condition, it is only necessary to float portions of any of the common mosses on a glass of water, which should then be kept in the shade. Radicles of

[1] 'Journal of Microsc. Sc.' 1862, p. 97.

considerable length are soon pushed out, and one of them may be removed for microscopical examination. From the examination of such filaments Dr. Braxton Hicks ascertained that the chlorophyll and protoplasmic contents not unfrequently detached themselves altogether from the cell-wall, and collected into one or more ovoid masses of different sizes. These at first possessed all the optical characters of living healthy vegetable protoplasm; but they soon began to change in colour from green to red or reddish-brown, and then gradually became so colourless that no trace of either red or green remained, except in the form of a few reddish granules[1]. Whilst these changes were advancing, the several masses gradually began to 'alter their form, and to protrude and retract processes exactly as *Amœbæ*.' Dr. Hicks says:—'They travelled up and down the interior of the cells, occasionally elongating themselves into a linear form. The movements of their contents presented the same phenomena as those of true Amœbæ.' And although all the masses of green endoplast generally passed through these changes simultaneously, this was not always the case. The number of amœboid bodies to be found within each cell is dependent upon the number of masses into which the cell-contents originally divides, and also upon the number of segmentations which these may subsequently undergo. Dr. Hicks has seen as many as seven Amœbæ moving about within a single cell. He says:—'Anxious

[1] These appearances are represented in Pl. iv., loc. cit.

to observe what became of these bodies, I carefully watched one for some hours, and observed the following:—First, the movement by protrusion became gradually restricted till it was extinguished, the mass returning to the ovoid form it possessed originally. The exterior also seemed to become more rigid, although I do not think there was any distinct cell-wall. Secondly, the whole exterior became covered with very minute cilia in constant vibration, by which the mass was kept in a state of agitation within the containing cell. The total motion was curtailed, of course; but in bodies which I noticed moving in the water undistinguishable from them, the motion was rapid and rolling. Beyond this point I was unable to extend my observations on their life-history.' This change from the amœboid to the ciliated state was rapidly effected, since Dr. Hicks had seen it brought about in the course of two hours [1].

We will now refer to some observations, previously alluded, to and made by Mr. H. J. Carter, which also furnish us with particulars of the greatest interest.

At pp. 187–190 of vol. i. we described the early changes taking place within an internode of a *Nitella* which is about to die. We then quoted Mr. Carter's description of the mode of formation of the 'gonidial' cell, as he at that time called it, and have now to follow his

[1] Dr. Gros had, however, long before—and even anterior to the date of Pringsheim's observations—declared that the nuclei of the spiral bands of *Spirogyra* might individualize themselves and become converted into Monads which subsequently developed into different forms of Ciliated Infusoria. (See 'Bullet. de la Soc. de Nat. de Moscou,' 1851, p. 477.)

description of the changes taking place within this cell, and of the ultimate metamorphoses undergone by the liberated cell contents.

The previously green contents having become of a brownish colour, and the transparent cell-wall fully formed, Mr. Carter says[1]:—'A new substance, consisting of a bluish semi-transparent mucus, more or

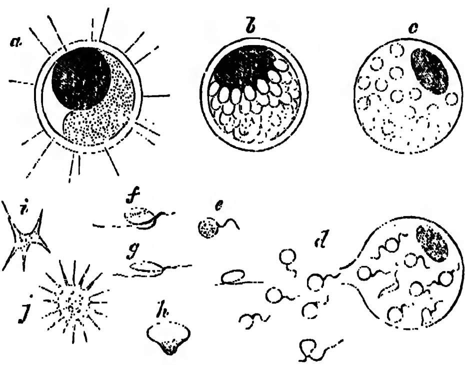


FIG. 75.

Heterogenetic Origin of Monads from Nitella. (Carter.) (× 350.)

a. Contents of new-formed Cyst separating into protoplasm and dark brown refuse matter.

b, c, and *d.* Segmentation of the protoplasm into Monads, which afterwards escape from the ruptured Cyst.

e, f, g. Different forms of the Monads.

h, i, and *j.* Forms of Amœba and Actinophrys which the Monads subsequently assume.

less charged with minute granules (from which its colour appears to be derived), and refractive globules of

[1] 'Ann. of Nat. Hist.' vol. xvi. p. 5.

a faint yellowish-green and sapphire blue colour, makes its appearance in different parts of the brown mass, or to one side of it, and afterwards, becoming botryoidal or mulberry-shaped, separates into gonidia. The brown chlorophyll with the other effete contents then shrinks up into a structureless, homogeneous, more or less defined, circular nucleus, of a dark brown colour, and the cell frequently projecting on one side in a conical form bursts at the apex and gives exit to the gonidia. . . . The gonidia are globular, ovate or spindle-shaped, and of a light blue colour. They average $\frac{1}{1300}''$ in diameter[1], and contain, together with the blue substance mentioned, more or less also of the refractive globules, and a transparent vesicle. Each gonidium is provided with one or two cilia, according to its form, that is to say, the globular ones present one and the spindle-shaped

[1] Although this is the average size, gonidia may be met with varying much in bulk. Mr. Carter says, p. 10:—'I have already described the commonest form of gonidium, but there is still another about twice the size, viz. $\frac{1}{2150}''$ in diameter, which, although not so frequent, is nevertheless sufficiently so to show that there are two sizes more common than the rest; for we shall presently see that the gonidial substance may occasionally come out as a whole, or in gonidia of all sizes below its original bulk. This large gonidium generally presents itself under a circular or globular form, with a single cilium, but it is sometimes seen ovate or spindle-shaped like the smaller one. It must be obvious to all, that a polymorphic cell, such as the gonidium is, can have no constant figure while in a state of activity; hence at one time it may be of one shape and at another of another, but when under polymorphism and the cilium has disappeared, a group of gonidia will exhibit a strong tendency to exhibit the same kind of figure generally, whatever that may be.' But just after they become stationary the form of *Actinophrys sol* seems to prevail.

two, which may be perceived whilst they are yet grouped or separate in the transparent gonidial cell, where they already exhibit a certain amount of polymorphism. Shortly after they have become free in the internode, the wall of the latter gives way and they pass into the water, where for a certain time they remain so active that it is almost impossible to describe their form; but here and there, that which I have stated may be seen in those which are less active in their movements than the rest.' The elongated or spindle-shaped forms are generally provided with two cilia of about equal length, one of which, usually motionless, is bent backwards underneath the body, whilst the other projects anteriorly and exhibits a constant whipping movement. But, 'After a while, perhaps some hours, the gonidia become stationary, and while they appear to be fixed by the proboscis mentioned, the long cilium floats motionless, or presents a languid kind of whip-like undulation; the latter then disappears, and a day or two after, the gonidia, both small and great . . . are seen creeping about the watch-glass (into which they were transferred for observation) under as active polymorphism as any amœbous cell could present; diffluent, digitated, and in the form of that beautifully radiated figure called *Actinophrys sol* (Ehr.).'

Again, we are told by M. Nicolet[1] that in vegetal substances which are undergoing decay Amœbæ are

[1] See Thompson's 'Arcana Naturæ,' Paris, 1859, p. 31, Pl. iii. figs. 1 and 2.

produced by the formation of a globule of glutinous matter or protoplasm, which first isolates itself, and then, leaving the substance of which it previously formed part, develops fine rays on its surface, which often attain a considerable length. Nicolet says:—'The Amœba lives under this form for several days, and takes the name of Actinophrys. It has been called *Actinophrys sol*, when its substance is purged from the colouring matter, which it almost always swallows soon after its formation.'

And according to Nicolet, the changes which take place in some of the lowest aquatic animals after death, when they remain immersed in their medium, are very similar to those which occur in a piece of decaying Spirogyra or other fresh-water Algæ. A similar production of Amœbæ takes place from altered portions of the dead organisms. He says[1]:—'As soon as the internal substance of the body of the animal becomes modified by decomposition, portions of the glutinous matter become isolated, and form globules of different sizes, more or less coloured by the food particles which had remained in suspension in the liquid. These globules issue slowly from the dead body, drawing themselves out, and sometimes leaving a portion of their substance behind; they then follow the line of development I have previously indicated.' They be-

[1] Loc. cit., p. 31, and Pl. ii. figs. 9–18, in which M. Nicolet represents the origin of such organisms from a dead Tardigrade, and also some of their subsequent changes.

come more or less encysted, and at the same time develop rays from their exterior. Nicolet adds:—'The different phases of their development has given rise to the establishment of many species. I will cite, amongst others, under the Actinophrys form, *Actinophrys viridis* (Ehr.), *Actinophrys digitata* (Duj.), and *Actinophrys difformis* of the same author; and under the Amœba form—*Amœba inflata* (Duj.), *Amœba diffluens* (Ehr.), *Amœba brachiata* (Duj.), *Amœba radiosa* (Ehr.), and *Amœba princeps* (Ehr.), species considered to be distinct by all authors, but which disappear and appear again in one and the same individual during observations conducted for a single month.'

Again, according to Nicolet, when a portion of an aquatic plant such as Chara has been well cleaned with a soft brush—so as to detach all foreign matters from its surface—and is placed in a vessel with some pure water, a variable number of minute transparent filaments may be seen to grow from its surface. The time at which these appear varies in different cases; the rapidity of their formation being in direct proportion to the lowness of vitality of the plant, and also, within certain limits, to the daily atmospheric temperature. They generally make their appearance on its surface as little transparent, hemispherical projections about $\frac{1}{2500}$ in diameter, which appear to contain an extravasation of some of the liquid contents of the plant itself. These projections grow very rapidly into transparent filaments—even in the

course of a few minutes—each of which, after it has attained a length of about $\frac{1}{250}''$, gradually develops a terminal, spherical, or ovoidal dilatation, which becomes

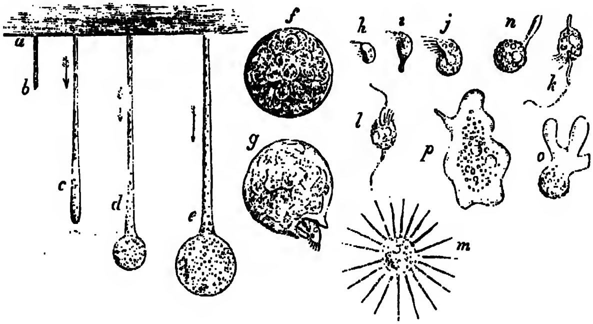


Fig. 76.

Mode of Origin of *Trichomonas*, and its Transformation into *Actinophrys* and *Amœba*. (Reduced, from Nicolet.)

a–e. Different stages in the formation of a germinal globule.
f. g. Segmentation of its contents into embryo specimens of *Trichomonas* (*h, i, j*).
k, l, n. Forms subsequently assumed by these bodies, which, later still, become converted into *Actinophrys* (*m*) or *Amœba* (*o*)—the latter rapidly increasing in size (*p*).

filled with a granular mucilage, poured into it through the tubular portion[1]. Soon, however, fluid ceases to

[1] Mr. Archer describes in 'Journal of Microsc. Sc.' 1860, vol. viii. p. 227, the production of somewhat similar tubes from a portion of a large Desmid, through which ciliated zoospores are subsequently discharged. These appear to be produced by a breaking up of the endochrome of the frustule itself, near the part whence the tube issues. When discharged the zoospores were found to be $\frac{1}{3000}''$ in diameter, and provided with a single cilium. They, in fact, very closely resembled the bodies whose production Mr. Carter had watched within the so-called

ascend, and the tube itself gives up its contents to the globule. The latter is often about $\frac{1}{800}''$ in diameter; its substance, perfectly homogeneous and much more refractive than water, remains quite motionless. But this stage of rest is one of short duration, and scarcely two minutes elapse before the different refractive powers of different portions of the contents show that a change is taking place. Signs of the internal differentiation of globular portions of the contents soon become evident, and grow more and more distinct. At last movement recommences—no mere displacement of the liquid, but an actual swarming movement of the spherical portions into which the contents have been resolved. The movements of the bodies—about fifteen or twenty in number—increase in extent and rapidity, till at last the envelope bursts and gives exit to these active units as ciliated organisms, similar to those which were included in the genus *Trichomonas* by Dujardin (*g*). Their bodies after a time become nodulated, and about $\frac{1}{2500}''$ in diameter. They are somewhat irregular in shape, and are prolonged at one extremity into a well-marked flagellum, at the base of which and on one side seven or eight ciliæ are situated. At the opposite extremity of the body there is a much shorter prolongation.

'gonidial-cells' of Chara. Mr. Archer believes them to be normal, though very unusual, 'zoospores' of the Desmid in question. Much doubt, however, still hangs over the subject. It seems to me more likely that they are heterogenetic products, and that they do not at all belon to the natural developmental cycle of the plant.

M. Nicolet states that this form, resembling Dujardin's *Trichomonas vaginalis*, speedily passes on to the development of an Amœba, resembling that which was named after Gleichen by Bory de Saint Vincent. After its escape from the globule in which it was formed, it swims about for a time with the aid of its vibratile cilia, then fixes itself by means of its caudal filament, either to the surface of the plant or to the walls of the vessel in which it is preserved, where it continues to oscillate, owing to the continuous agitation of its cilia. It soon begins to project amœboid expansions, which are generally long and simple, though after two or three days it detaches itself from its pedicle, and swims about for a time before again coming to a state of rest. 'Its body then becomes quite spherical, whilst it loses its locomotory cilia, and becomes bristled with straight and slender rays, which give it the appearance of an *Actinophrys*.' This new form (*m*) often lasts less than a day. 'The rays then disappear by retraction, the body flattens itself, becomes discoid, and soon protrudes new expansions on all sides, though these are larger, less regular, and resemble those of *Arcellæ*. Shortly afterwards the disc extends itself on one side into rounded lobes, and an Amœba is formed. For a time the more granular substance, in which the vacuoles form, remains agglomerated posteriorly; though later, the granules diffuse themselves throughout the whole mass, and then this Amœba, named after Gleichen, has no longer any characters by which to distinguish it from other Amœbæ.'

The interest attaching to these observations of M. Nicolet is extremely great, though other observations of Mr. H. J. Carter, to which we shall now allude, are just as startling. Nicolet does not appear to have been aware of these, though they were made known more than two years before his own were published. It was in a paper communicated to the Bombay branch of the Royal Asiatic Society[1] in November, 1856, entitled 'Transformation of the Vegetable Protoplasm into *Actinophrys*,' that Mr. Carter first published these very important observations. The transformations had, for the most part, been witnessed taking place within the cells of *Spirogyra crassa*, one of the filamentous fresh-water Algæ, and about the largest representative of its genus.

He says:—'Under certain circumstances, the cell of *Spirogyra* apparently dies, the chlorophyll becomes yellow, and the protoplasm leaving its natural position divides up into portions of different sizes, each of which encloses more or less of the chlorophyll; these portions travel about the cell under a rhizopodous form, the chlorophyll within them turns brown, the portions of protoplasm then become actinophorous, then more radiated, and finally assume the figure of *Actinophrys*.

[1] And reported in 'Ann. and Mag. of Nat. Hist.' 1857, vol. xix. p. 259. Concerning the facts themselves Mr. Carter has never varied, and he is well known to be a most conscientious and trustworthy observer. At different times, however, he has been inclined to explain these and other observations differently (see 'Ann. of Nat. Hist.' 1861, pp. 285-288).

The radii are now withdrawn; while the pellicula in which they were encased is retracted and hardened into setæ, with the rest of the pellicula, which now becomes a lifeless transparent cyst. A more delicate cyst is then secreted within this, and the remains of the contained protoplasm, having separated itself from the chlorophyll, segments into a group of monociliated Monads, which sooner or later find their way through the cysts into the cell of the *Spirogyra*—the latter by this time having passed far into dissolution, so that the Monads afterwards easily escape into the water[1].' This was the process seen by Mr. Carter when the cells of *Spirogyra* were not (as they are just before conjugating) loaded with starch. But when the changes took place at the period of conjugation they were somewhat different. The whole of the contents of the two conjugating cells became united into one mass, and having assumed a globular form, remained in this state until the chlorophyll had become more or less brown. After this the protoplasm reappeared at the circumference of the sphere in two forms, viz. in portions which leave the mass altogether after the manner of Amœbæ, or else in the form of tubular outgrowths which continue to maintain their connexion with the sphere. In both instances the protoplasm is without chlorophyll, but charged with oil globules; and both forms make their

[1] The dissolution of the filaments is a slow process of thinning, not putrefactive in nature; indeed Mr. Carter says:—'Putrefactive decomposition at the commencement destroys this process altogether.'

way towards the confines of the *Spirogyra* cell, which they pierce before any further elaboration of their contents takes place.

On reaching the cell wall, each form puts forth a minute papillary eminence, which, having passed through the wall, expands into a 'large sac, or bursts at its apex.' The isolated form gradually drags four-fifths or more of its bulk through the opening, and sometimes so much is dragged through as only to leave a little papillary eminence within—so that the portion of protoplasm seems as if it were entering instead of escaping from the *Spirogyra* cell. Whereas in certain cases where a tubular extension is formed, it expands into a delicate cyst of a flask-like or globular shape, outside the cell wall, and retains the protoplasmic contents here until they are ultimately developed into Monads. These, which are much larger than the Monads developed within the Nitella filaments, on issuing, move about rapidly for a time by the aid of a strong cilium carried in front like that of *Astasia*, and then become stationary. The vesicula, or 'contracting vesicle,' which does not appear before they leave the cyst, now becomes very active, the cilium is gradually diminished in size and altogether disappears, and the 'Monad passes into a rhizopodous reptant state, which afterwards becomes actinophorous, and finally assumes a form indistinguishable from that of *Actinophrys sol.*' The developmental changes taking place within these outgrowths, and the subsequent changes which the pro-

ducts undergo, bear therefore a close resemblance to the metamorphoses witnessed by M. Nicolet (pp. 383-386).

Mr. Carter has also described the mode of origin or appearance of certain 'tubulating germ cells,' which seem closely to resemble the peculiar fungoid growth

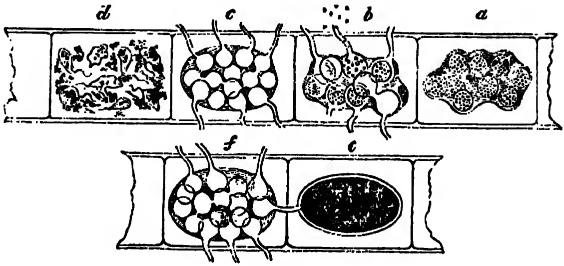


Fig. 77.

Formation of Pythium and of Astasiæ within cells of Spirogyra. (Reduced from Carter).

a. Mode in which Pythium first appears.
b. Corpuscles with tubes, through which the contained granules are voided.
c. Empty corpuscles after discharge of granules.
d. Formation of Astasiæ within a contiguous compartment of Spirogyra.
e. A spore of Spirogyra, into which one of the tubules of an inosculating group of corpuscles (*f*) is penetrating.

described by Dr. Pringsheim[1] as *Pythium entophytum.* This is represented as a colourless, flask-shaped or pyriform organism, with a more or less elongated neck; and it is stated to occur in both animal and vegetal tissues. Mr. Carter says[2]:—'Just after the conjugation of *Spirogyra*, a number of spherical cells filled with minute

[1] 'Ann. des Sc. Nat.' (Bot.), 4 Sèr. t. xi. pl. 7, fig. 1.
[2] 'Ann. of Nat. Hist.' vol. xvii. p. 113.

refractive granules frequently make their appearance within the mucus-layer of the cell; and when the former shrinks from the latter, these spherical cells become wrapped up in it.' Each of the spherical cells usually develops a blind tube, which sometimes penetrates the cell wall of the Spirogyra for the exterior liberation of its contained germs, though others of them may communicate with adjacent cells. And occasionally one of these inosculating spherical cells may send its tube 'through the septum of the cell into the resting-spore of the next cell, which, being full of nutritious matter, immediately furnishes food for the whole brood.' Mr. Carter says that the granules of the tubulating cells are of different sizes, and motionless at first; though they subsequently 'become locomotive, swarm about the cell, and then pass out of the tubular prolongations.' Cells of an altogether similar character were also seen to develop within the dead bodies of certain Rotifers[1].

[1] Concerning the actual origin of these products Mr. Carter has exhibited much vacillation of opinion. Thus, although at one time he believed they were formed from a modification of the substance of the organism in which they appeared, he afterwards renounced this view and adopted his first notion that they were parasites whose germs had been introduced ('Ann. of Nat. Hist.' 1861, pp. 285–288). But tubulating germ cysts of a somewhat similar nature have been seen by Stein within *Vorticella microstoma* and *Vorticella nebulifera* (see Pritchard's 'Infusoria,' p. 357); and they were thought by Stein to represent one of the modes of reproduction of these Infusoria—which seems to show that they appeared to him to be formed from the very substance of the organism in which they were found. Cienkowski has also observed a similar development of brood cells within encysted specimens of *Nassula viridis*

In addition to the presence of these peculiar tubulating cells in Spirogyra, Mr. Carter frequently found many *Astasiæ* either in the same or in different filaments; and owing to the supposed absence of other means of accounting for the presence of these organisms, he hazarded the not very convincing guess that they may have been derived from some of the liberated germs of the tubulating cells. The important point, however, is Mr. Carter's statement of the fact that 'young *Astasiæ* are also developed within the cells of *Spirogyra* to a great extent.' He says they at first exhibit almost as much polymorphism as an Amœba, though after a time they assume the form and exhibit the movements peculiar to *Astasiæ*.

On other occasions Mr. Carter has seen peculiar filaments appear within the closed cells of Spirogyra. There is, he says, 'frequently a development of long, slender, colourless filaments, which have a writhing movement like that of an injured earth-worm,' and some of the filaments present a faint appearance of segmentation. Mr. Carter also states that such bodies may be met with in Desmids. He says:—'The same kind of filaments occasionally appear in *Closterium acerosum* when its contents are passing into dissolution, but long before the chlorophyll has changed colour or

(Pritchard, pl. xxviii. figs. 65–71); whilst Claparède and Lachmann have seen the same kind of organisms appear within certain non-encysted Infusoria ('Ann. of Nat. Hist.' vol. xix. p. 238). The subsequent fate of the liberated particles (germs) is very uncertain and needs further investigation.

putrefaction has commenced.' Mr. Archer has also described and figured[1] a mycelial growth found within a *Closterium lunula*, which was very similar to the organisms just referred to[2].

In speaking of the prevalence of one or other of the modes of development already described, Mr. Carter says they are common in Chara and Nitella, and in Cladophora and Spirogyra; that they occur occasionally in Hydrodictyon, and also in Closterium and Cosmarium among the Desmidiæ, though never in the Diatomaceæ. He has also frequently met with such changes in Euglenæ, and in the dead bodies of Furcularian Rotifera; and he adds:—'The same or similar developments probably take place throughout the whole of the fresh-water Algæ, and in many of the Infusoria.'

Changes very similar to, though not precisely the same as those already described, have been frequently watched by the author in Nitella, Vaucheria, and other Algæ. And those who work at this subject must not

[1] 'Journal of Microsc. Science,' 1860, pl. xi. fig. 6.

[2] I have myself very frequently seen these growths within large specimens of Closteria, though the filaments have always been quite motionless. They seemed to be formed out of the substance of the Desmid, and always first manifested themselves near the clear central portion at a time when no alteration of colour had taken place. I have occasionally found that their presence could be determined at will by simply keeping the Desmids for a time beneath a covering glass, or in a small unventilated chamber. Under these conditions also, fungoid growths of various kinds will make their appearance within the filaments of Spirogyra, Vaucheria, or other Algæ; whilst other heterogenetic changes which may have been previously taking place become arrested.

necessarily expect to be able exactly to confirm the observations of others. They will soon gain an insight into the rich variety of possible transformations, and will recognize the difficulty of obtaining at will an exact repetition of many of those which they have themselves observed.

Nothing surprised me more, on the very first occasion on which I examined one of the common species of Vaucheria, obtained from a road-side ditch, than to find how easily many of the phenomena which have hitherto been described might be observed. The marvel was how so many of the naturalists who had been investigating these Algæ could have failed to appreciate the real nature of the changes which they must have seen so often. We may constantly observe, within a dying filament of Vaucheria, aggregations of protoplasm and of chlorophyll vesicles, of various sizes, some of which are quite irregular in outline whilst others are spherical and with or without an enveloping membrane or cell wall. ome of these masses are as much as $\frac{1}{500}''$ in diameter. After a time, the chlorophyll gradually loses its colour, and all intermediate stages may be recognized between such spherical masses and others in which the central region is stained by a deep brown colour, whilst the peripheral portions are more colourless and homogeneous. In this latter stage the masses seem to have undergone two principal modifications. A certain number of them have become encysted, the cyst wall being very thick; whilst others present

no trace of a cyst wall, and even in this stage their clear peripheral portion is seen to be made up of veritable protoplasm, which is continually undergoing slow alterations in shape. These masses are already more than half-formed Amœbæ, and that they have been produced out of mere fortuitous aggregations of chlorophyll and protoplasm is beyond all doubt. All intermediate changes may be seen taking place even in different parts of a small portion of one of the filaments, and the examples are so numerous as to make this almost simultaneous observation of the different stages just as conclusive as if we were to attempt to follow out all the developmental phases of any one of the masses.

The filaments of different specimens of Vaucheria vary a good deal as regards the amount of protoplasmic matter and chlorophyll which they contain; and some specimens are also much more prone than others to exhibit the changes to which we have just referred. When a healthy filament is cut across and placed on a slip, beneath a covering glass, for microscopical examination, its contents slowly exude, and as it emerges it separates into green masses of different sizes, which almost immediately take on a spheroidal form. When these masses are examined by a magnifying power of 1600 or 1700 diameters, they are found to be composed of a semi-fluid protoplasm, containing granules of different sizes and also a number of bright green chlorophyll vesicles. Slight contractile amœboid move-

ments are seen to take place in the masses, causing movements to and fro of the contained granules and vesicles, and also producing extremely slight and temporary irregularities in the outline of the sphere. We have, in fact, to do with artificially separated masses of the algoid protoplasm which are already more than half amœboid in nature.

The changes that take place in an unhealthy filament are therefore not difficult to understand. When the conditions are unfavourable for the continuance of the life of the Alga, portions of its protoplasm of various sizes become individualized within the filament; and such a change may simultaneously affect the whole of the protoplasmic contents of a certain length of filament, so that on microscopical examination it may be seen to have become arranged into spheroids of various sizes, from $\frac{1}{2000}''$ to $\frac{1}{500}''$ in diameter (*a*). In this stage the chlorophyll vesicles within the spheres may be perhaps of a brighter green than usual, whilst here and there, in some of the spheres, they may have begun to decolourize by assuming various tints of olive-green. In this stage the individualized masses are quite motionless, or at most they merely exhibit some very slight alterations in the disposition of the diaphanous protoplasm existing at their surface (*c*). Others of them, however, whilst their contained chlorophyll vesicles are still unaltered, protrude a few pseudopodia, by the languid contraction and extension of which the masses very slowly move from place to place (*b*). On

two or three occasions bodies (about $\frac{1}{1800}''$ in diameter) which seemed to have had the same origin, and in

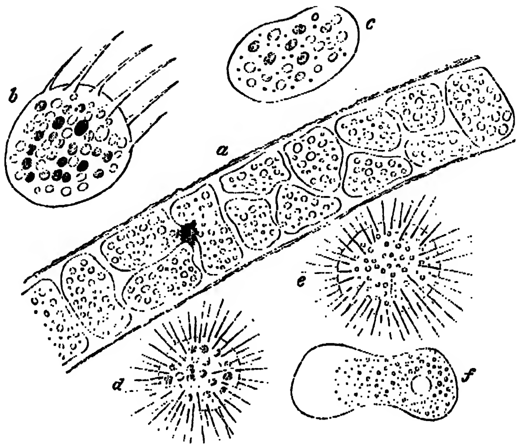


FIG. 78.

Transformations of the substance of Vaucheria. (× 600.)

a. Portion of small filament in which contents are individualizing themselves into amœboid masses (diagrammatic).

b. One of masses containing green chlorophyll moving slowly by means of pseudopodia.

c. Almost motionless amœboid mass—with partly decolourized chlorophyll.

d. Similar mass with numerous motionless rays.

e. Similar body in later stage as an active and wholly decolourized Actinophrys.

f. Another mass after retraction of its ray assumes the habit and movements of an Amœba.

which the chlorophyll was still unchanged, were seen moving about pretty actively within the filament; though it was not ascertained whether these move-

ments were effected by the aid of a flagellum or by cilia.

Gradually, in the course of the second day, the chlorophyll within these rudimentary amœboid or Actinophrys-like masses passes through various stages of transformation, by which the green matter becomes converted into colourless protoplasm. During the process the Actinophrys-like bodies become more and more active, and the number of rays notably increases (*d*), whilst the substance of these organisms becomes more granular and vacuoles develop in their interior. On the third or fourth days the process of decolourization may be complete; and no one, looking at these thoroughly animalized organisms (*e*) for the first time, would be inclined to believe that such active, many-rayed specimens of Actinophrys could, but four or five days before, have constituted portions of the green matter of one of the filamentous Algæ. Such specimens of Actinophrys rapidly increase in size, though after a few days some of them may retract their rays and become transformed into active Amœbæ (*f*). Certain of the green spheres may also pass directly into Amœbæ—the substance of the masses becoming more and more animalized as decolourization advances. Other spheres may form a condensed outer layer or cyst-like envelope, within which the enclosed masses of protoplasm undergo their various final stages of decolourization—terminating in the evolution of different higher forms of life to which we shall subsequently refer, since they may also present

themselves during the transformation of other matrices.

Meanwhile, I will pass to a description of somewhat analogous metamorphoses which have been frequently observed to take place within the closed internodes of Nitella.

A few filaments of *Nitella translucens* were kept in a small glass vessel, containing about a quarter of a pint of water from the pond in which the plant had been found[1]. It was protected from dust, and kept upon the mantelpiece of my study during the months of September to February inclusive; and the changes which were from time to time observed within these filaments were of the most interesting nature. The plant literally 'died by inches'—internode after internode—and the changes which took place within them were found to vary considerably according to the temperature, the brightness of the weather, and other undiscovered influences[2]. The initial changes were, however, very similar in all cases, although they progressed at different rates.

When an internode just beginning to grow pale was examined, phenomena very similar to those described by Mr. Carter might be observed. The protoplasm and chlorophyll vesicles were seen to have aggregated into masses of various sizes, from $\frac{1}{1428}''$ to $\frac{1}{300}''$ in diameter.

[1] It had been sent to me from Falmouth by Mr. Howard Fox.

[2] It also grew by inches, since new internodes were constantly sprouting out as the old ones died.

The bright green vesicles were massed together in the midst of a clear and almost motionless protoplasmic substance, which was always quite evident at the periphery (Fig. 79, *a*). The included chlorophyll corpuscles, in the course of a few hours, began to decolourize, passing through the usual shades of olive green and brownish tints, till the whole mass became converted into almost colourless protoplasm. During this process it happened in some cases that Actinophrys-like rays were pushed out from the clear border, by means of which the masses very slowly moved from place to place. As the process of decolourization went on more rays were emitted, and the creatures became more and more active, whilst vacuoles developed in their interior—so as to form (after forty-eight hours or less) most perfect specimens of Actinophrys, altogether similar to those which are formed in Vaucheria filaments. Their subsequent fate was not accurately determined, although thousands of them were sometimes present at the same time within a single dying internode.

More frequently, however, another change was seen. During the process of decolourization the masses remained spherical and almost motionless, and pushed forth no rays. They soon became finely granular bodies, having a yellowish brown colour in the central parts, though the edge of the masses (either all round or partly round their circumference) had become completely decolourized and more refractive in appearance (*b*). Sometimes these spheres contained a blackish brown,

residual granular mass near the centre; whilst at other times the process of conversion had been more effectual, and had left no remainders. These bodies we may, for the sake of convenience, henceforth speak of as *embryonal spheres.*

Once formed, such spheres may undergo very various changes, and I shall begin with a description of those which, as regards temperature and season, take place under the most unfavourable conditions.

1. The internal substance of the embryonal spheres may after a time gradually become fluidified, so that the granules and other particles are seen in active movement in their interior. The moving particles gradually increase in number and in size, till at last the whole internal contents of the mass has been resolved into thousands of Bacteria, held together only by the thinnest superficial bounding layer—which, however, soon gives way and discharges its swarm of simplest living units into the filament.

2. Some of the spherules may become perfectly decolourized and finely granular in their interior, and then may develop a blind tube, so as to produce large specimens of Pythium (*f*, *f′*). In other bodies, which appear to be essentially similar during their early stages, the contained granules gradually aggregate into large nuclear masses, each of which subsequently becomes enclosed within one of the segments into which the contents of the sphere divides (*e*). These segments soon become active and liberate themselves in the form of

globular Monads. Other decolourized vesicles, instead of developing a blind tube, protrude a hemispherical outgrowth, from which beautiful tree-like ramifications of delicate branches extend (*d*). These three forms seem to be most intimately allied to one another—judging from the ease with which apparently similar vesicles may take on now one now another of such modes of transformation.

3. Or if the temperature happens to be not lower than 60°F, the whole substance of the embryonal spheroid may at once segment into small and very active non-nucleated Monads (*c'*). This segmentation begins at the periphery in the colourless portions of the spheroid, whilst subsequently the more central parts gradually undergo a similarly complete decolourization, and segmentation of them also goes on. Sometimes the most superficial layer is involved in the transformation, so as to leave no bounding membrane, and then in the course of an hour or so, when the Monads begin to exhibit movements (owing to the development of the flagellum), they may swim away one by one. But at other times a very thin, cyst-like, outer layer is left, so that after the whole of the contents has been transformed into Monads, they may be seen in pretty active movement before they succeed in rupturing their delicate enveloping membrane. In some instances (*c*) a mass of blackish brown granular matter remains as a refuse product (especially in cases where the change has not gone on very actively), though in other spheroids the whole of the matter undergoes conversion.

4. The supervention of less favourable conditions during the progress of the last-mentioned change will

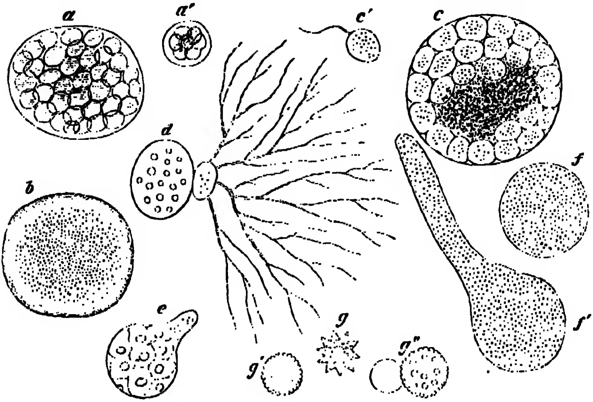


FIG. 79.

Lower Transformations of the substance of Nitella. (× 600.)

a, a'. Amœboid masses of different sizes, densely packed with bright-green chlorophyll corpuscles.

b. A similar mass after decolourization—constituting an 'embryonal sphere.'

c. An embryonal sphere segmenting into Monads (*c'*), but leaving a dark-coloured refuse-heap.

d, e, f. Three interchangeable developments of smaller embryonal spheres: *d*, a sporangium giving birth to an arborescent outgrowth; *e*, a sporangium which subsequently segments into Monads; *f*, a large kind of Pythium which subsequently develops its usual blind tube (*f'*).

g, g', g''. Three forms of Sporangia which are occasionally formed from the central portions of embryonal spheres.

put a stop to it, and cause change No. 1 to proceed. In this case, whilst the peripheral portions of the embryonal sphere are metamorphosed into Monads, the

central portion becomes resolved into thousands of active Bacteria.

5. At other times, whilst the external portions of the embryonal sphere break up into myriads of embryo Bacteria, the central portion escapes this change and becomes transformed into a Fungus-sporangium about $\frac{1}{3000}''$ in diameter, sometimes like *g* and sometimes like *g′* or *g″*.

6. Very rarely it happens that, after complete decolourization, the whole mass becomes more plastic and finely granular, and creeps about in the form of a large vacuolated Amœba (Fig. 80, *a*), which undergoes rapid changes of shape, and may subsequently assume the form of an Actinophrys (*b*).

7. Or the whole mass having become colourless and highly refractive, it assumes a slightly ovoid form, gradually becomes more animalized in its composition, pushes cilia from its surface, and thus becomes bodily converted into an embryo of one or other of the various forms of Ciliated Infusoria (*c c′*, *d d′*). At other times (as in the case of the transformation into Monads) the mass may undergo change within a very delicate cyst formed by the condensed outer layer of the spheroid. The cyst ultimately gives way near one of its extremities, so as to permit the exit of a young Oxytricha—an infusorial form which is almost invariably to be found within dead though still closed internodes of Nitella[1].

[1] Further details concerning the origin of Oxytricha (pp. 467–469) and other Ciliated Infusoria will be found in the next Chapter.

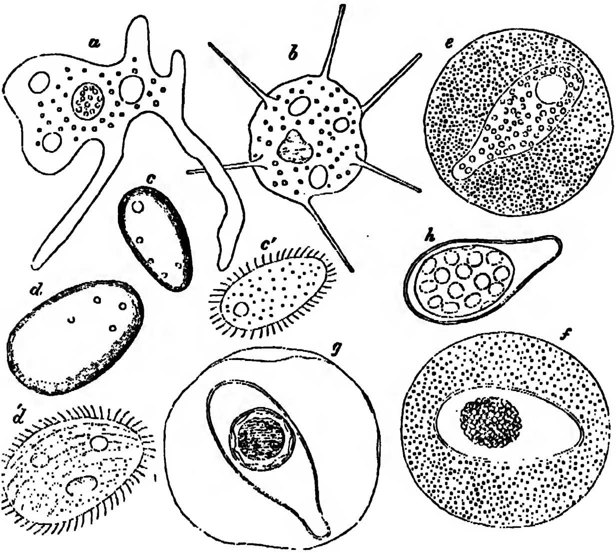


FIG. 80.

Higher Transformations of the substance of Nitella. (× 600).

a. A large Amœba resulting from the transformation of a single embryonal sphere.

b. Form of Actinophrys which such an Amœba may subsequently assume.

c. Small embryonal mass which has assumed an ovoid form, and subsequently becomes concerted into a kind of Paramecium (*c′*).

d. Similar mass, rather larger, which becomes flattened, and subsequently develops into an embryo Chilodon (*d′*).

e. A complex egg-like body into which individual embryonal spheres were very prone to develop; the central mass of protoplasm containing large granules and a vacuole.

f. Later stage of same body—central mass of fatty-looking globules, which subsequently becomes homogeneous and enclosed within another cyst, as in *g*.

h. Later stage of development as seen in a few embryos, which may or may not have escaped from their loose outer envelope.

8. On other occasions nearly all the embryonal spheres within a given internode may, almost imme-

diately after they have been formed, convert themselves into complex egg-like bodies, whose nature has not yet been determined (*e*, *f*). These bodies vary as much in size as the embryonal spheres—which is only natural, seeing that they are but modified forms of such spheres. What determines their conversion into this form is just as mysterious and beyond our ken as are the causes which induce arsenic to crystallize in the form of beautiful octahedra. In both cases we are thrown back upon the actual facts which, however real they may be, we are utterly unable to explain. We can only say that both sets of phenomena are dependent upon molecular composition and the unknown laws of polarity. Hundreds of such complex egg-like bodies may occasionally be seen within a single compartment of a verticel, whilst the contiguous compartments on each side may be lined by as many of the 'embryonal spheres' similarly variable in size, which, instead of becoming converted into a single germ of some higher form, are undergoing segmentation into the before-mentioned Monads. But seeing that such different transformations are met with under the influence of the same conditions, the difference must, as we have indicated, depend upon the precise nature and vigour of the molecular movements taking place in the algoid matter of the respective compartments.

The formation of these complex egg-like bodies seems to represent a higher transformation than any which we have as yet recorded in this series.

We shall subsequently have occasion to refer to other series of changes in which—as in these embryonal spheres and in the embryonal areas of the pellicle—the portions of matter undergoing metamorphosis, may either segment into one or other of various lower forms of life, or may each be transformed bodily into a single large organism.

Some remarkable transformations have also been observed taking place in individual chlorophyll corpuscles which still remained *in situ* within the filament. A careful examination of these chlorophyll corpuscles soon suffices to convince the observer that they are veritable independent units. They grow, in fact, and frequently undergo spontaneous division after the fashion of an algoid corpuscle or gonidium, as Nägeli long ago ascertained. It is not at all uncommon to see some of the corpuscles within dying filaments of Nitella become of a bright grass-green colour and increase very much in size, growing more or less ovoid or irregular in shape, and exhibiting certain irregular markings in their interior. The later modifications which corpuscles affected in this particular manner undergo have not been ascertained, though changes of a different nature have been followed out much more completely.

An uninjured Nitella internode, two and a half inches in length, had been placed with some water in a corked test-tube. It retained its vitality for a long time,

and on examination at intervals, many changes were observed which it is unnecessary now to particularize. At the expiration of five weeks the filament was found to have become rather suddenly decolourized—with the exception of a few patches of green here and there, in which the chlorophyll corpuscles were still regularly disposed[1]. Within the filament myriads of small colourless Actinophrys-like bodies were seen, and a careful examination of some of the patches of chlorophyll corpuscles revealed all intermediate stages between individual corpuscles and the colourless stellate animal bodies. The different stages were seen in hundreds of corpuscles lying side by side in such a way as to make the order of change just as obvious as if the same corpuscles had been watched through the several stages.

The corpuscles in the patches were generally of a decidedly paler green colour than natural, though others which were about to undergo the animal metamorphosis had become of a brighter and darker green than natural, and soon began to exhibit a slightly granular condition of their contents (Fig. 81, *b*). At the same time these dark green corpuscles seemed to have increased somewhat in size. They moreover continued to become larger till they were from $\frac{1}{2500}''$ to $\frac{1}{3000}''$ in diameter. They also became more ovoid and more granular—some of the granules being green and others colourless (*c*). At this stage a vacuole appeared in their interior, and

[1] Three or four days previously a much larger quantity of the chlorophyll layer had been present.

Actinophrys-like rays were protruded—the corpuscles still remaining quite motionless, and a few of the granules being still green (*d*). The latter soon became quite colourless; whilst the rays grew rather longer (*e*), and at the same time began to exhibit slow and sluggish

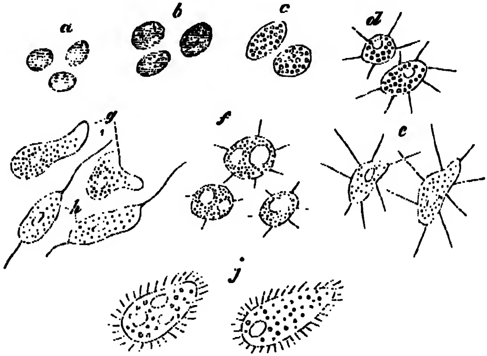


FIG. 81.

Transformations of Chlorophyll Corpuscles. (× 600.)

a. Pale, unaltered Chlorophyll Corpuscles of Nitella.
b. Others lying side by side with former, but larger, of a darker green, and slightly granular.
c. Decolourization advancing—a few granules still green.
d. Similar corpuscles after the protrusion of motionless rays, and formation of a vacuole.
e. Similar corpuscles completely decolourized and converted into sluggish specimens of Actinophrys.
f. First stage in transformation of Actinophrys, some of which are converted into Amœbæ (*g*), and others into Monads (*h*) with two flagella.
j. Enchelys-like organisms, probably derived from further development of some of Monads and Amœbæ.

movements. Some of the bodies appeared to exist in this Actinophrys-form for some time, though others

very quickly passed on to different conditions. Their rays became shortened, and large vacuoles appeared and disappeared in the interior of corpuscles which were now almost spherical (*f*). These spherical corpuscles partly transformed themselves into tolerably active Amœbæ (*g*) and partly into rather sluggish Monads (*h*), provided with a flagellum at each extremity, which, at the time of observation, were mostly vibrating very slowly.

Five days afterwards the above-named bodies had for the most part disappeared, though the filament then contained myriads of ovoid ciliated Infusoria about $\frac{1}{700}''$ in length, and of the simplest description (*j*)—closely resembling Dujardin's Enchelys or embryo Paramecia.

Observations of a somewhat similar nature have been detailed by Dr. Braxton Hicks[1], who saw the chlorophyll corpuscles of certain moss-radicles notably increase in size and become cellular, whilst their contents divided into three, four, or more motionless segments. The modified corpuscles remained for some months in this condition; though after this time, when some of them were placed in the sun, the contained segments seemed to be rapidly converted into very active, faintly-green, granular, and bi-flagellated Monads. The testimony of Dr. Gros, given long before, is also to the same effect. He says he has seen the chlorophyll corpuscles of large Euglenæ individualize themselves, greatly increase in size, and gradually become colourless and finely granulated.

[1] See *Appendix* D, p. lxxi.

They thus formed ovoidal bodies which speedily developed cilia over their whole surface, and became at once converted into Enchelys-like Ciliata[1], very similar in appearance to those above referred to, which seemed to have been ultimately produced from the metamorphosed corpuscles of Nitella.

At other times, according to Dr. Gros, the individual chlorophyll corpuscles of Euglenæ become the parents of various Confervæ and Oscillatoriæ. And, strange as it may appear that apparently similar products should at one time develop into animal forms and at another give birth to unmistakeable vegetal products, it is imperative that we should more and more familiarize ourselves with the notion of the frequency with which these interchanges occur. Many such cases will subsequently be mentioned, in addition to the instance now about to be cited, which we owe to the careful observations of Dr. Braxton Hicks, who, having already alluded to the circumstance that the chlorophyll corpuscles of moss-radicles had been seen to undergo transformations whereby they became resolved into a nest of Monad-like bodies[2], tells us in another place[3] that he has frequently seen masses of Gleocapsa developed from the older leaves at the base of the stem of many Mosses. These leaves frequently assume a brownish aspect in winter and spring, owing to the cell walls taking on this colour whilst their contents still

[1] Loc. cit., pp. 330 and 487, Pl. J, figs. 2 and 3.

[2] See *Appendix* D, p. lxxi. [3] 'Trans. of Linn. Soc.' 1862, p. 581.

remain green. But, as Dr. Hicks says, 'After a time the old cell wall dissolves away, and then it becomes evident that the contents have assumed the form of, or rather, have become a *Gleocapsa*, which certainly undergoes segmentation freely. . . . . I have seen considerable masses of *Gleocapsa* produced in this manner[1].'

But we have not yet mentioned all the changes which chlorophyll vesicles or the ultimate elements of Algæ may undergo. They may even lapse into modes of growth whereby Pediastreæ, Desmids, or Diatoms are produced. These remarkable metamorphoses were long ago pointed out by Dr. Gros, though his statements on this subject (in common with many others to which we shall subsequently allude) have been almost universally disregarded. And yet the paper in which his observations are recorded is probably one of the most important that has ever been published on a biological subject. His statements were made, however, whilst the minds of the majority of naturalists were enthralled and whilst their vision was perverted by mere theories—so that they could neither discover the truth for themselves nor give credence to the positive representations of one who had been able to approach the subject like a true student of nature.

Dr. Gros' own words on the subject of the origin of Desmids and Diatoms are as follows[2]:—'En 1845, nous

[1] Loc. cit., Pl. lviii. fig. 24.

[2] Loc. cit., p. 339. And it must be remembered that Dr. Gros wrote at a period when so much precision had not been given to the nomenclature

avions déjà signalé l'origine végétalo-animale des Baccillariens et des Navicules. Pour ces dernières, elles reconnaissent les origines en apparence les plus diverses; en effet, on les voit sortir de divers degrés de parifissure euglénienne en prenant des formes appropriées; on les voir sortir de la résolution des Conferves les plus diverses qui individualisent leur vésiculines; des Gromies qui résolvent le contenu de leur coque; des Mousses et autres végétaux dont les cellules poussent une sorte de végétation clostérienne (Pl. P, figs. 13, 16); des internœuds de conferves vigoureuses (fig. 18), en prenant les formes les plus artistiques, etc. Elles sortent toujours d'une vésicule individualisée, et deviennent libres ou restent en aigrettes (fig. 16) etc. Quant aux Baccillariens, (sans prétendre jamais que dans un fait se résument toutes les possibilités évolutives) on voit des conferves eugléniennes[1] (Pl. L, fig. 17), dont les nœuds (*a*) se scindent en formant une longue chaîne (*b*), qui se scinde encore (*c*), en se décolorant comme à l'ordinaire, en s'aplatisent à prendre plus de largeur (*d*), jusqu'à ce que les frustules se détachent (*e*), après avoir pris plus de dimension. . . . . . En thèse général, il est constant que jamais ni Navicule, ni Baccillaire, ni Clostérien, ou Diatomien n'a reproduit son semblable[2], que ces organisms sont un effet de la

of these organisms; and also, as he tells us (p. 295), in a region in which books of reference were not accessible.

[1] That is, Confervæ which have taken origin from the subdivisions of Euglenæ.

[2] Except by a process of fission.

division d'autres organismes, qu'ils sont une impasse et la fin d'une série vitale et rarement une forme de transition (Micrasterias, Arthrodesmus, etc.).'

Amongst the many special instances illustrating the truth of his views which are mentioned by Dr. Gros, there is one to which I will now call special attention, as I have quite recently observed transformations of an almost similar nature.

Green cell-like bodies which had taken origin from a Moss-leaf were, after a time, seen by Dr. Gros to become converted into colourless specimens of Actinophrys. These increased in size, and ultimately retracted their rays previous to developing cilia and becoming converted into one or other of the forms of Ciliated Infusoria[1]. Other specimens of the same cells underwent repeated subdivision, and their segments assumed the form of Arthrodesmus (see Fig. 85, *h*). Some of these four-segmented bodies were afterwards seen to separate into their elemental parts, and each of them divided obliquely (*j*) so as to form two ellipsoidal corpuscles, which speedily developed into some of the endless forms of Naviculæ[2].

My own observations were as follows:—Having

[1] Loc. cit., pp. 452 and 501.

[2] On the other hand, Arthrodesmus, Micrasterias, and other Pediastreæ, whatever may have been their origin, are said by Dr. Gros to lapse into the confervoid mode of growth whenever they are placed upon a damp soil (loc. cit., p. 452). At p. 311 Dr. Gros also speaks of the origin of Arthrodesmus from the fission-products of Euglenæ, and of these being converted in the manner above stated into Naviculæ.

placed a few filaments of Vaucheria in a watch-glass protected by an inverted wine-glass, I found, two or three days afterwards, that a thin scum had formed upon the surface of the fluid [1]. On microscopical examination, the most notable constituents of the scum were certain motionless, green corpuscles, varying from $\frac{1}{3000}''$ to $\frac{1}{850}''$ in diameter, and containing ovoid chlorophyll vesicles (Fig. 82, *f*). These bodies were evidently undergoing transformations in different directions. Many of the smaller corpuscles were becoming decolourized, and were protruding rays so as to convert themselves (as the Nitella vesicles had done) into specimens of Actinophrys, which subsequently assumed the forms of Monads or Amœbæ. Some of the larger corpuscles, however, gradually decolourized, so as to undergo higher transformations, in a manner which will be subsequently detailed [2]. In other smaller corpuscles the vesicular contents, instead of fusing and becoming decolourized, underwent an extra amount of individualization. The chlorophyll vesicles increased in size and became of a slightly brighter green, whilst the thin investing membrane seemed gradually to dissolve away, leaving bright green, rounded or ovoidal corpuscles, about $\frac{1}{8000}''$ in diameter (*g g'*), which subsequently

[1] These corpuscles seemed to grow first upon the surface of the Vaucheria filaments.

[2] Leading very frequently to the production of Vorticellæ. Corpuscles given off from other Algæ have been observed to go through similar transformations.

passed through one or other of various changes. Occasionally two or three of them, after having very slightly increased in size, became ovoid and rather paler at one extremity, before protruding a flagellum and moving about actively as green Monads. Other of the ovoidal corpuscles continued to increase in size, at the same time becoming more and more fusiform, whilst their green contents became granular (*g–h'*). The most elongated of these were subsequently bisected by a delicate partition; they also developed a greenish nuclear-like body in each segment, and soon began to grow into unmistakeable filamentous Desmids, of which many otherwise similar specimens were seen in all stages of growth (*h'–k*). But other representatives of the minute ovoid corpuscles assumed a paler colour, and then a slightly olive tint, whilst their colouring matter became in part metamorphosed into two comparatively large, rounded, nuclear corpuscles. These bodies increased in size, and it soon became obvious that they were young Naviculæ (*l*, *l'*). The exact pattern assumed in the early stages is subject to much variation, and several different kinds of Diatoms seemed to be produced corresponding to these different initial forms (*m*, *m'*). At first no striation was observable, but gradually their envelope became more and more differentiated—silica appearing to be assimilated from the water in which they were immersed—and some of these Diatoms exhibited a well-marked striation.

Occasionally the individualizing contents of one of

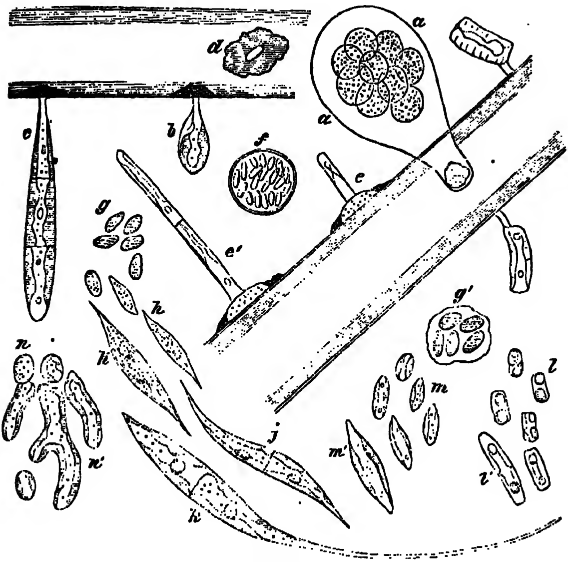


FIG. 82.

Modes of Origin of Desmids and Diatoms.

a. Green algoid corpuscles contained within a hyaline envelope which buds from a Cladophora filament. (× 600.)

b, c. Outgrowth of Desmids from the surface of Nitella filament—the base being surrounded by a dark brown zone, which persists (*d*) after the Desmid has disappeared. (× 600.)

e, e'. Other filamentous Desmids, springing from a green thalamus which is surrounded by the brown zone. Pediculated Diatoms were also seen budding from the same Cladophora filament. (× 600.)

f. An algoid vesicle (× 600) budded off from Vaucheria, whose elongated corpuscles frequently increased in size and became liberated from their envelope (*g*), after which some of them grew into Desmids (*b, b', j, k*). Other vesicles (*g'*) gradually become converted into different kinds of Diatoms (*l, l', m, m'*). (× 1750.)

n, n'. Origin of Desmids from chlorophyll corpuscles of Vaucheria. (× 600.)

the green vesicles containing eight corpuscles ranged themselves in one plane and in close apposition, so as to constitute what appeared to be an embryo Micrasterias. In this, as in other respects, the transformations of these vesicles were similar to those which have been observed to take place amongst Euglenæ (see Fig. 85, *l*.)

Individual chlorophyll vesicles of Vaucheria and Nitella may also gradually metamorphose themselves into Desmids. I have seen this change take place with the greatest distinctness in some of the corpuscles of the Vaucheria. They enlarge and become of a pale green colour, and whilst this colouring matter limits itself to a surface layer, a few colourless granules appear in the central portion of the vesicles (Fig. 82, *n*, *n'*.) These vesicles gradually elongate so as to form rudimentary filaments, which after a time give off lateral buds and develop dissepiments at intervals[1]. At other times, various kinds of Desmids originate most plentifully from Algæ and Characeæ by a different process. A minute tubular bud appears at some portion of the surface of the filament, which as it enlarges acquires green contents. It rapidly grows into some kind of filamentous Desmid—some of the specimens being very narrow and others broad, and of a very bright

[1] Dr. Gros has evidently watched the same mode of transformation in vesicles derived from Euglenæ. Speaking of these, he says:—'On peut ajouter encore que ces parcelles vésiculaires continuent à s'organiser pour devenir des Clostériens aigues.' (Loc. cit., p. 302.)

green colour (*e, b.*) These filaments sometimes appear to spring at once from the outer layer of the filament, and sometimes from an expanded base, also having a greenish colour, which soon becomes surrounded by a dark brown decolourized zone (*e, e'.*) The zone often persists long after the Desmid itself has disappeared. At other times I have seen attached to the same kind of base (on specimens of Cladophora) a large ovoidal and perfectly hyaline envelope containing in its centre an aggregation of bright green algoid spherules (*a*), each of which seems to be capable of enlarging and transforming itself into an Astasia. Diatoms also appear to grow from the surface of the filamentous Algæ, to which they are afterwards seen to be attached by a hyaline tubular pedicle; and it is this mode of origin, apparently, to which Dr. Gros referred when he said [1]: 'C'est une chose soupçonnée sinon reconnue par tous les observateurs que des vésicules vertes se font jour à travers le tube des Conferves, et se convertissent en Navicules.' Although many Algæ present phenomena of this kind, in none are they more striking than in certain specimens of Vaucheria. Filaments of this weed which but a few days before—when placed for observation in a watch-glass—may have showed neither Diatoms nor Desmids upon their surface will at times become crowded, both inside and out, with a rich variety of both of these modes of growth. And although it is quite possible that in all such instances the

[1] Loc. cit., p. 316.

Desmids, Diatoms, or algoid corpuscles may be due to the growth of almost invisible germs derived from previously-existing similar organisms, one is bound to state on the other hand that such germs are never recognizable in the situations upon which the outgrowths subsequently appear, and that the postulation of their existence is an assumption based upon no independent evidence.

What is at present known concerning the modes of reproduction of Desmids and Diatoms[1], is wholly inadequate to account for their sudden appearance in great numbers, in situations where they did not previously exist. And in the face of the actual transformations which have now been witnessed by independent observers, whereby algoid or euglenian corpuscles are bodily converted into Diatoms or Desmids, it is rendered all the more probable that the bud-like method of origin is as independent of pre-existing Desmid or Diatom as it seems to be. But concerning the transformations of pre-existing corpuscles of a different nature into such bodies, we shall have more to say in the next chapter.

On other occasions certain of the Protococcus-like corpuscles which are so frequently given off from many Algæ may, instead of multiplying after the fashion of Confervæ, increase in size and gradually exhibit an animal-like activity, whilst still retaining their green colour. They thus become converted into Astasiæ and

[1] See Pritchard's 'Infusoria,' 4th ed., pp. 11 and 58.

Euglenæ, bodies which may also have other and quite different modes of origin[1] Astasiæ, for instance, have

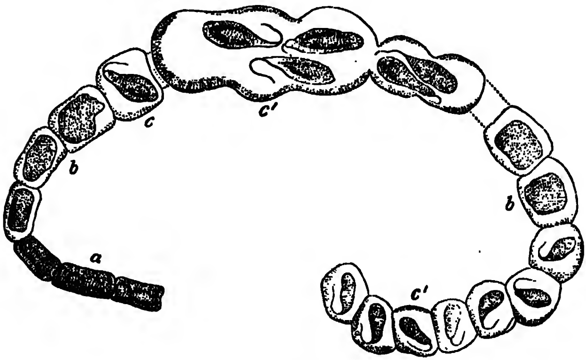


FIG. 83.

Origin of Euglenæ from the Cell-contents of a Conferva. (Gros.) (× 250?).

a. Unaltered Cells of the Alga.
b, b. Separation of the Cell-contents and first stage of individualization.
c, c. Individualized masses which have assumed the form of Euglenæ.
c' Similar organisms, somewhat increased in size, and contained within a dilated chamber formed by the obliteration of several dissepiments.

been seen by Mr. Carter to originate within the closed cells of certain filaments of Spirogyra which were under-

[1] See Dr. Gros' Memoir, loc. cit., p. 289. The difference between these two forms is quite unimportant, and there is reason to believe that Astasiæ frequently develop into Euglenæ. Both are green plastic vesicles, which usually move about by means of a long anterior flagellum; and the Euglena differs from the Astasia principally by its possession of a spot of red pigment near the origin of the flagellum. In addition to the fact of the presence of chlorophyll in their interior, these animal-like forms are also more related to the products of the vegetal kingdom by reason of their mode of nutrition. They never take visible portions of food into their interior.

going change[1]. Whilst Dr. Gros[2] has seen the contents of each internode of an unnamed species of Conferva separate from the walls and aggregate into a single mass, which gradually assumed the form and characteristics of a Euglena (Fig. 83). These somewhat animalized organisms were subsequently liberated, owing to the hyaline walls of the algoid compartments becoming thinner and thinner and ultimately rupturing.

I have also seen remarkable changes of a similar kind taking place within the small cells composing one of the submerged leaves of a species of Potamogeton[3]. All the chlorophyll and protoplasm, in some of these cells, became aggregated into a spherical mass; whilst other uninjured cells were seen, each of which contained a large bright-green Euglena, having the usual red pigment speck at one end. The bodies of these creatures were as mobile as Amœbæ, and they were continually moving around within their narrow prisons. They were also, in each case, the sole occupants of the cell—the whole of the chlorophyll and protoplasm of which had evidently been newly embodied into the form of an Euglena.

And lastly, Prof. A. M. Edwards is so certain about the derivation of Euglenæ from Confervæ, that he says[4],

[1] See p. 392, and Fig. 77.

[2] Loc. cit., p. 490, description of Pl. K, fig. 9.

[3] Heterogenetic changes take place quite freely within the cells of almost all aquatic plants when they get into an unhealthy condition.

[4] 'Proceed. of Lyceum of Nat. Hist.,' New York, vol. i. (1871), p. 215.

'I am convinced it will be at some future day shown that all the green, and some of the red colored forms similar to Euglena, and which have had several names bestowed upon them, are but transition states of fresh-water or marine Confervoid Algæ.'

But seeing that metamorphic changes of the most surprising nature occur in individual masses of algoid protoplasm, would it not be reasonable to suppose that Astasiæ or Euglenæ may also, at different times, undergo a number of heterogenetic changes, leading to the production of totally different forms, both animal and vegetal. And, as a matter of fact, such phenomena were long ago stated to occur. The accurate observations of Dr. Gros, which we shall have to quote in the next chapter, do of themselves fully suffice to show that such organisms easily pass, at different times or under different conditions, into the most diverse representatives now of the Animal and now of the Vegetal Kingdoms.

Although we may be astounded by the changes recorded in this chapter—at the very high forms, and at the diversity of the living things which are evolved, as compared with those which arise in the pellicle on organic infusions—it may also be seen that these differences do not remain wholly unaccountable. Whether we have to do with one of the lower aquatic animals, with an Alga, or with any other submerged cryptogam, actual portions of its living matter, as such, undergo

certain metabolic changes constituting the first steps towards the production of new organisms. On the other hand, the organic matter in the infusion exists in a state of solution, and certain primordial living units must appear in it, must aggregate and fuse, before distinct masses of living matter can exist, in which secondary metamorphic changes may take place leading to the production of higher organisms. Amongst such aggregates, also, it would seem likely that more of uniformity would exist than in portions of the actual tissue of a plant or of an animal shortly after it had begun to die. The mere size or bulk of the masses which are submitted to this simultaneous change has been shown, in both cases, to have much influence over the kinds of organisms that are produced. The higher forms are almost always evolved from the larger masses, unless, from some unknown cause, an accidental segmentation of the mass has been initiated—though then, again, the same law is exemplified, since, instead of one large and more or less complex organism being produced, many small and comparatively simple creatures are evolved. It would seem that in the larger mass, made up as it is of living matter of extreme instability, there is a wider field for, as well as an increased liability to, the occurrence of those successive molecular differentiations which must occur in the production of higher organisms.

There is still another cause, to which we have not yet adverted, which doubtless strongly favours the occurrence

of some of the more striking metamorphoses. It must be recollected, that in all the aquatic plants in which these changes have been noticed, the chlorophyll corpuscles become incorporated with the protoplasmic substances, and so help to constitute the spherical masses out of which the new organisms are to be evolved. But chlorophyll is a most complex and unstable body, well calculated to excite even more metabolic changes amongst the protoplasm than would otherwise occur[1]. M. Frémy describes it as a substance, 'd'une excessive mobilité,' so that its mixture in different proportions, leading to slight differences in the molecular changes induced, would be likely to give rise to variable results in the metamorphic processes taking place within the same vegetable cell or algoid filament, or within similar filaments on different occasions[2].

[1] These masses of matter are indeed so unstable and so prone to undergo change that I have found it quite impossible to preserve them unaltered as microscopical specimens—although I have tried almost every known means of mounting them. They soon lose their colour and all their characteristic appearances; and yet when the tissues of higher animals are mounted in some of the same fluids they will remain comparatively unaltered for years.

[2] 'Compt. Rend.' 1865, p. 188. According to Frémy, chlorophyll is a peculiar sort of coloured fatty substance which undergoes a kind of saponification under the influence of bases; leading to the production of *phylloxanthine*, a yellow neutral body, and a bluish-green fatty acid, which he proposes to name *phyllocyanic*. This latter substance, insoluble in water, is soluble in sulphuric and hydrochloric acids—producing liquids which, according to circumstances, may be green, reddish-violet, or of a most beautiful blue colour. M. Frémy says:—' Voici donc un acid retiré de la chlorophylle et qui par l'action de certains réactifs peut

Thus several reasons are discoverable why the changes taking place in the matter of aquatic organisms should give rise to much more varied and also to much higher metamorphic products than those commonly derived from the 'pellicle.' The developmental phases here encountered are indeed very comparable with some of those which have been already described in *Appendix* D as definite changes in the life-history of many of the lower forms of life. And what has since been made known does much to strengthen the supposition then advanced[1]. Instead of looking upon many of these sets of changes as definite series which were always likely to occur in the same order when similar organisms were observed at different times, it was suggested that the existing testimony of skilled observers, not only pointed to the conclusion that no such regularity was observable, but also rather tended to favour the supposition that we had to do merely with a living matter which—far outdoing the fabled Proteus—was capable of assuming an almost endless diversity of living forms, under the influence of varying changes in its own substance, and various modifications in the nature of its environment[2]. Before

prendre des colorations vertes, violettes, ou bleues. . . . C'est là le fait important qui me parait dominer ce travail, et qui pourra servir à expliquer les différentes teintes qu'offre la chlorophylle dans la végétation.'

[1] This Appendix was written two years ago, when I was unaware of the possible occurrence of many of the heterogenetic transformations which have now been recorded.

[2] See *Appendix* D, pp. xcvii. and lxxxiii.

these changes all early botanical and zoological distinctions seemed to vanish. Our notions of species, genera, families, orders, and even other more general classificatory distinctions, appeared to be swept away, one by one, in the face of the successive modifications and metamorphoses which these simple organisms were capable of undergoing.

CHAPTER XXI.

TRANSFORMATIONS OF EUGLENÆ AND OTHER ORGANISMS: MODES OF ORIGIN OF CILIATED INFUSORIA.

Crystals and Organisms. Variability of latter. Derivative Organisms. Observations of Dr. Gros and of Author upon Euglenæ. Their Resolution into Fungus-germs and Monads. Resolution of other Euglenæ into Diatoms and Algoid Corpuscles. Transformations of entire Euglenæ into Diatoms, Desmids, and Pediastræ. Transformation of others into Confervæ. Interchangeability of Algæ and Lichens. Relations of Algæ to Mosses. Observations of Dr. Gros and M. Brébisson. Community of Nature between Algæ, Pediastreæ, Desmids, and Diatoms. The latter form a Divergent Series. Transformations of Euglenæ into Amœbæ and Actinophrys. Their subsequent Development into Ciliated Infusoria. Direct Transformations of Euglenæ into Ciliated Infusoria. Variable Nature of resulting Forms.

Other Modes of Origin of Ciliated Infusoria. Transformations of Chlorococcus Vesicles into Oxytricha and Plæsconia. Similar Mode of Origin of Vorticella. Development of latter also from bud-like outgrowths, and from the 'pellicle.' Origin of other Ciliated Infusoria from Monads and Amœbæ. Testimony of various Observers. Dependence of Forms upon the size of Transforming Matrices. Observations of M. Nicolet upon Chara. Mode of Origin of Otostoma within Nitella-filament. Origin of almost similar forms of Infusoria from Animal Matrices. Pangenesis in Rotifers. Their Resolution into Actinophrys, Peranemata, and Arcellinæ. Tendency of these Forms to give rise to Ciliata.

Other Modes of Origin of Arcellinæ. Origin of Ciliated Infusoria from eggs of Gasteropods and Rotifers. Other Modes of Analytic Heterogenesis in Rotifer 'eggs.'

THE observations we have already recorded afford abundant evidence as to the readiness with which mere formless living matter takes on what biologists have been led to regard as quite specific living shapes—shapes of a kind which have hitherto been considered as the accumulated products of modifications that have been going on in one ancestral line for ages [1]. The facts are so new and strange that, even now, many of them would seem almost incredible to ourselves, if their truth and reality had not been guaranteed by the testimony of our own senses. As it is, however, all we can do is frankly to admit their occurrence, although, for the present, they are more or less beyond our comprehension. An investigation of the changes which took place in the 'proligerous pellicle' of organic solutions compelled us to assert that a Paramecium might actually come into being *de novo*, with all its specific characters, in a few days. And this statement has since received the

[1] How much the facts are opposed to what has been anticipated may be judged by comparing them with the comparatively recently-published statement of the most distinguished exponent of the Evolution philosophy, who says:—'The evolution of specific shapes must, like all other organic evolution, have resulted from the actions and reactions between such incipient types and their environment. To reach by this process the comparatively well-specialized forms of ordinary *Infusoria* must, I conceive, have taken an enormous period of time.' (See Herbert Spencer's 'Principles of Biology,' Appendix, pp. 480, 481).

strongest confirmation by other investigations, which have taught us that similar Ciliated Infusoria, of various kinds, may arise just as rapidly by reason of changes taking place in a formless protoplasmic substance, which but a few days previously existed as an integral portion of a living plant, and performed all the functions pertaining to its vegetal nature. What are we to say under such circumstances? Is it possible to look upon the resulting Infusorial animalcule as aught else than the living morphological representative (or resultant) of the conjoint action of the molecular polarities of its constituent organic atoms, under the influence of the physical forces which are at the time operative? The rationale of their form and structure cannot differ, so far as principle is concerned, from that similar explanation which alone can be adduced to account for the appearance, in a saline solution, of any complex crystalline form, such as a doubly oblique rhombic prism or other highly-specific crystalline type. Both the Crystals and the Infusoria must be regarded as the direct products of the series of actions and interactions which have taken place between the materials of which they are composed and the medium or environment in which they exist[1]. How such different products may arise

[1] Professor Huxley says:—'It is not probable that there is any real difference in the nature of the molecular forces which compel the carbonate of lime to assume and retain the crystalline form, and those which cause the albuminoid matter to move and grow, select and form, and maintain its particles in a state of incessant motion. The property of

in saline solutions and in organic infusions respectively —products endowed with such totally different tendencies—we may perhaps dimly see our way to comprehend, if we take into consideration the fundamental nature of the difference existing between ordinary saline materials and the diverse big-atomed colloids of which living things are compounded.

The interchangeability of animal and vegetal modes of growth—so strikingly illustrated in the last chapter—was long ago recognized by a few eminent naturalists [1], though systematists have never been wanting in energy or will to denounce, what they considered, such revolutionary and anarchical doctrines. The additional evidence now about to be adduced will, however, suffice to set the final stamp of truth upon the views of those who regard Animals and Plants as mere modes of growth of a fundamentally similar living matter—which, though at first it may assume more or less neutral forms, is ever ready, now under one set of influences to go along the higher animal modes of development, and now under another to persist in the simpler vegetal modes of nutrition.

Several naturalists, however, have also expressed their conviction that many of the lower forms of life—

crystallizing is to crystallizable matter what the vital property is to albuminoid matter (protoplasm). The crystalline form corresponds to the organic form, and its internal structure to tissue structure. Crystalline force being a property of matter, vital force is but a property of matter.' ('Fortnightly Review,' Feb. 1869.)

[1] See Lindley's 'Veget. Kingdom,' 3rd ed. pp. 2 and 8.

both animal and vegetal—possess or are endowed with a natural tendency to develop into higher forms. Thus Kützing, in his prize essay on the Transformations of Plants, asserts, according to Mr. Berkeley[1], 'that from one and the same organic material, even when it has acquired form and colour, different vegetable [organisms] may be developed, which, according to the circumstances of the surrounding medium, are Algals, Fungi, Lichens, or Mosses; and that even the spores of these, when produced, are capable of generating plants belonging to different orders.' Whilst elsewhere[2], after stating that simple Algæ under certain circumstances 'may raise themselves to vegetations of a higher form,' he expressly affirms that 'the same superior formation may be produced by primitive formations of altogether different kinds.' Again, Prof. Reissek[3] says he has seen Confervæ arising from the metamorphosed chlorophyll vesicles of ordinary flowering plants, and that he has also observed similar forms produced by the development, under unusual conditions, of pollen-grains. Similar views have been announced by Meyen[4] both as to the diverse modes of origin of the same kinds of Lichen, and as to the convertibility of different forms. Such views are, moreover, confirmed by the observations of Dr. Braxton Hicks,

[1] 'Introd. to Cryptogam. Botany,' 1857.
[2] 'Ann. des Sc. Nat.' n. s. vol. ii. p. 225.
[3] 'Bot. Zeit.' July 19, 1844.
[4] 'Ueber die Entwickelung &c. der Flechten.'

Itzigsohn, and many others[1]. And, moreover, it is stated by the Rev. M. J. Berkeley that the common edible Mushroom is cultivated by gardeners with as much certainty as any other vegetable, although no seeds are ever sown. It is only necessary that beds should be prepared in a certain fashion, and then this complex Agaric almost infallibly appears. Mr. Berkeley says the process is 'so certain, that no one ever saw any other kind of Agaricus produced in mushroom-beds—except a few of the dunghill tribe, where raw dung has been placed near the surface of the bed;' and he adds, 'this could not happen if the mushroom sprang from seeds or sporules floating in the air, as in that case many species would necessarily be mixed together[2].' These facts are almost inexplicable unless we resort to the belief that the lower forms of Fungi may arise by heterogenesis[3],

[1] See *Appendix* D, pp. liii–lix.

[2] It will afterwards be seen that almost precisely analogous facts have to be recorded concerning the appearance of Nematoids in prepared mixtures (p. 537), and the same difficulty as that which Mr. Berkeley experienced with regard to the derivation of Mushrooms from atmospheric germs is applicable to the origin of the germs of the Rotifers, Tardigrades, and Nematoids, which are always to be found in tufts of Moss and Lichen.

[3] After quoting Fries's objection to this view, on the score that the small size and infinite number of the sporules of Fungi permitted of their being widely disseminated through the air, the Rev. M. J. Berkeley very fairly remarks (Lindley's 'Veget. Kingdom,' p. 34):—'I give his words as nearly as possible, because they may be considered the sum of all that has to be urged against the doctrine of equivocal generation in Fungi; but without admitting, by any means, so much force in his statement as is required to set the question at rest. In short, *it is no answer to such arguments as those just adverted to.*'

and then supplement this belief by the notion that more complex forms may afterwards be developed from them.

Whilst, therefore, many observations have already been made tending to establish the existence of a most intimate relationship between Fungi, Algæ, Lichens, and Mosses, and to show that many of them tend to push on to higher developmental forms, it has also been positively ascertained that very many of them are constantly giving birth to animalized organisms—such as Astasiæ, Euglenæ, Amœbæ, Monads, and Ciliated Infusoria. It now remains to show that these various derivative organisms exhibit a similar, though even more strongly marked, tendency to develop into higher forms—both gradually and by means of sudden and startling transformations.

And of all the animalized forms given off by the lower vegetal organisms, none are so remarkable, or possess within themselves such marvellous potentialities for undergoing change, as the beautiful green Astasiæ and Euglenæ (Fig. 84, *a*), which occur so abundantly in ditches and other stagnant waters. More than twenty years ago these changes were carefully studied by Dr. Gros, and the principal results of his investigations were given to the world in the highly important memoir from which I have already quoted. He showed that they may give birth to the most varied animal and vegetal forms; and whilst struck by the apparent caprice which seemed to regulate the opposite transformations of specimens lying side by side, and of other specimens at different

seasons or when exposed to different amounts of light and heat, he was compelled to acknowledge that the causes of these differences lay wholly beyond our powers of observation. However much such facts might seem to be contradicted by generally-received theories, Dr. Gros, like a true student of Nature, said:—'Les théories peuvent avoir leur valeur, mais elles doivent servir à illuminer la série des faits, sans nous éblouir ni nous aveugler.' And yet Dr. Gros has, for the most part, been referred to as a visionary and misguided investigator, by critics who have immorally thrown doubt upon the truth of his statements—they, at the time, being almost wholly swayed by mere theoretical considerations.

Although my own observations upon Euglenæ have been conducted during the months of January, February, and March, and therefore at a period of the year which is not very favourable either for obtaining large specimens or for the occurrence of the higher kinds of transformations, these observations have nevertheless, as far as they have gone, tended in almost all respects to confirm those of Dr. Gros. In many respects, also, the changes which the Euglenæ pass through are analogous to the transformations already described as occurring in Nitella, Vaucheria, and other Algæ.

I shall commence with a description of the processes of Analytic Heterogenesis which have been observed to take place in Euglenæ, and shall subsequently speak

of the transformation of entire organisms into the most varied vegetal or animal forms of life.

1. *Resolution into Fungus-germs.* Some specimens of Euglenæ having been placed in a 'live-box' without sufficient ventilation, after twenty-four hours several of them were seen to be undergoing decolourization and developing Fungus-germs in their interior. They had all assumed a spherical form, and most of them were undergoing the same kind of change. Some of them contained a variable amount of brownish black matter, derived from a metamorphosis of a portion of the chlorophyll; though intermixed with it there were to be seen certain unaltered chlorophyll vesicles, as well as a number of colourless corpuscles, about $\frac{1}{10000}''$ in diameter, which, judging from what might be seen in contiguous Euglenæ, were evidently Fungus-germs. For in many of these Euglenæ only a small amount of pale-green matter still remained distributed amongst the colourless spherules with which they were now filled; whilst in others, several of the Fungus-germs had given birth to large filaments, which grew outwards and flourished externally in all directions.

2. *External Vesiculation, with Resolution into Monads or Fungi.* This change is one of those which I have most frequently observed taking place amongst Euglenæ, after they have existed for some time in a motionless state as constituents of a Euglena-pellicle, and when the intrinsic and extrinsic conditions are not sufficiently

favourable for higher changes. The contents of individual Euglenæ lose their distinctly corpuscular character, and at last become obscured by a brownish granular matter resulting from a decomposition of the chlorophyll. Meanwhile, a colourless outgrowth forms from some portion of the surface of the vesicle, and gradually increases in size (Fig. 84, *c*). This outgrowth varies much in shape. It may be spheroidal or irregularly cylindrical, and is often more capacious than the Euglena from which it is derived. The matter which it contains is colourless, semi-fluid, and evidently derived from the transformation of the substance of the Euglena—for as the outgrowth increases in size, the Euglena gradually disappears, till at last nothing of the old organism remains except the thin investing membrane. The semi-fluid contents of the outgrowth are not homogeneous: from the first there exists, diffused through all parts of it, a variable quantity of solid refractive matter, which seems to be derived from a curdling of its semi-fluid substance (*d*). This solid matter exists in the form of irregular granules, either separate or arranged in serial aggregations variously uniting with one another. But such irregularly disposed matter gradually aggregates round definite centres, so as to form a number of solid, refractive, nuclear spherules, pretty uniformly distributed throughout the whole mass (*e*). After a time the matter itself in which they are imbedded begins to undergo segmentation, in such a way that each one of the nuclear masses becomes included within an inde-

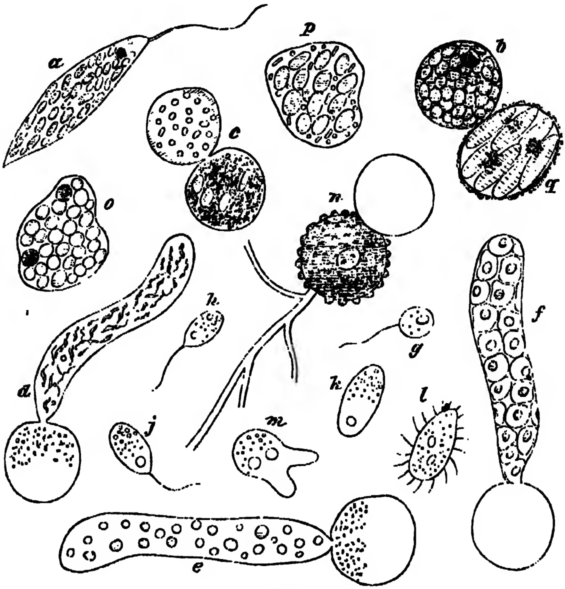


FIG. 84.

Resolution of Euglenæ into smaller Organisms. (× 600.)

a. Euglena in its active state.

b. Euglena contracted and about to encyst itself.

c. Colourless spherical outgrowth from transforming Euglena.

d. Subsequent stage—outgrowth elongated and containing irregularly disposed solid matter, which gradually aggregates itself into spherules (*e*), each of these afterwards becoming enclosed within one of the units into which the mass segments. These subsequently liberate themselves as active Monads (*g*), which gradually assume the forms of *h*, *j*, and *k*; whilst the latter motionless form either develops cilia (*l*), or else becomes fluent and moves about as an Amœba (*m*).

n. Another form of outgrowth from transforming Euglena—constituting a brown, tuberculated Fungus-sporangium.

o. Resolution of Euglena into Monads, partly green.

p Resolution of Euglena into Algoid Corpuscles and Bacteria.

q. Resolution of Euglena into Diatoms.

pendent unit, all of which soon begin to exhibit slight signs of movement (*f*). The united movements of the contained units soon rupture the delicate investing membrane, from which they emerge as small but active flagellated Monads or Zoospores. They appear as almost transparent spheres about $\frac{1}{3500}''$ in diameter, each of which contains a large nuclear mass at its posterior extremity, and swims by means of the vibration of a single anterior flagellum [1]. Their ultimate fate will be subsequently referred to [2].

Under less favourable conditions these nucleated vesicles are not liberated as flagellated Monads, but as motionless corpuscles, which also protrude motionless, ray-like prolongations, capable of enlarging into tubular filaments and branching in various directions. They grow, in fact, after the fashion of a Fungus-germ; and the nuclear mass within the germinal vesicle becomes subdivided into two or three smaller portions, whilst the vesicle itself enlarges. Wherever the filaments issuing from these bodies come into close contact with Euglenæ, they seem to penetrate; and, by virtue of some more coercive molecular movements, or vital changes, they gradually alter the constitution of the Euglena itself, so as to make it also take on the fungoid mode of growth.

[1] Dr. Gros has evidently seen a similar change take place (see loc. cit., p. 312, Pl. D, figs. 13-16, 21). And, although he does not expressly state that the Monads arise in an outgrowth, he has in other parts of his memoir spoken of the formation of such outgrowths.

[2] See p. 472.

3. *External Vesiculation leading to the Production of a Brown Spherical Vesicle with a Nodulated Surface—Nature uncertain.* Under circumstances similar to those last mentioned, some specimens of Euglenæ undergo decolourization, and produce a spherical outgrowth of their own size, which rapidly becomes brown and nodulated over its whole surface (*n*). These bodies vary much as regards the intensity of the brown colour and their degree of opacity. They generally persist for a long time without undergoing any change. A nucleus may sometimes be recognized in their interior, and on several occasions I have seen two or three delicate filaments issuing from them as from a Fungus-sporangium. Bodies of a similar nature are also frequently produced from large Chlorococcus vesicles about $\frac{1}{1000}''$ in diameter.

4. *Direct Resolution into actively-swarming Monads.* This change has only been observed in a few samples of Euglenæ, though amongst one batch it took place very frequently. The organisms which underwent the change were motionless and more or less spheroidal in shape, though not encysted. Some specimens were seen whose whole contents had been resolved into a number of minute motionless spherules, which were about $\frac{1}{10000}''$ in diameter, partly of a pale green colour and partly colourless. In other Euglenæ only a portion of their substance had undergone this change—the remaining parts presenting the ordinary green corpuscles and red speck (*o*). But in a third set, all trace of the

normal contents of the Euglenæ had disappeared, whilst their thin investing membrane was seen to be densely packed with minute and now actively-swarming Monads, some of which were still partly green in colour. On the rupture of this membrane the liberated Monads were observed to be almost spheroidal, minutely granular, and provided with a single flagellum[1].

5. ***Resolution into Diatoms.*** I have only distinctly observed appearances indicative of this transformation on one occasion; but in this case the whole of the contents of a Euglena seemed to have been resolved into seven distinctly-striated Naviculæ (*q*). They were closely packed within the thickened envelope of the Euglena, which possessed no other contents save three or four small refuse aggregations of reddish brown granules. In close contact with this transformed Euglena there was another of the same size in its natural green state—so that the two might have been the products of a previous fission. Although the earlier stages of the transformation were not seen, I have no doubt that the Diatoms originated in this way. A somewhat similar mode of origin of some of the wedge-shaped

[1] Dr. Gros refers (loc. cit., p. 323, Pl. F, fig. 19 *a*, *b*) to a somewhat similar change which took place in specimens of Euglenæ which had been kept for some time under the microscope; and Mr. H. J. Carter ('Ann. of Nat. Hist.' vol. xvii.) has also observed somewhat similar changes. The internal contents of the Euglenæ became resolved into a uniformly granular substance, which then segmented into six or eight globular masses; but on the rupture of the investing membrane, 'the granular masses, being liberated, began to creep about under the forms of *Actinophrys*.'

Diatoms, usually existing in stipitate clusters and which go by the name of Gomphonema, has also been observed by Dr. Gros[1].

6. ***Resolution into Algoid Corpuscles.*** This change has been observed to occur very frequently in small Euglenæ. The chlorophyll corpuscles which they naturally contain increase in size, and at the same time assume a very bright, dark-green colour. And whilst this individualization of the contents of the Euglena is going on, its investing membrane gradually becomes thinner and thinner, so as at last to liberate these enlarged and bright-green corpuscles. They are then free to pursue an independent existence. Sometimes, whilst this change is taking place and when the membrane of the vesicle is still intact, a number of very active Bacteria may be observed distributed amongst the bright-green corpuscles, although no such organisms are to be seen in the fluid outside. They appear to have been produced from a retrograde change taking place in some portion of the matter of the Euglena which had not been absorbed by the growing chlorophyll corpuscles.

The subsequent fate of such corpuscles seems to be very similar to that of others which we have already

[1] Loc. cit., p. 324; Pl. H, fig. 2; and Pl. G, fig. 11. I have occasionally seen large Naviculæ densely packed within a portion of a young Vaucheria filament whose ordinary contents had in this part of its length wholly disappeared. See also Mr. Metcalfe Johnson's statements concerning similar mode of origin of Diatoms from Algæ, in 'Monthly Microsc. Journ.,' Jan. 1870, p. 31.

followed. Some of them become decolourized and converted into Actinophrys, Monads, and Amœbæ, after the same manner as the chlorophyll corpuscles of Nitella (p. 408); whilst others subsequently grow either as Algæ, Pediastreæ, Desmids, or Diatoms—changes which we have also followed in the corpuscles similarly produced from vesicles of Vaucheria origin (p. 415). My own observations on this subject are entirely in accordance with those of Dr. Gros, who speaks of the origin of Monads, Algæ, Pediastreæ, Desmids, and Diatoms from individualized and liberated corpuscles of Euglenæ [1].

We shall now turn to a consideration of the transformations which an entire Euglena may undergo; although before dwelling upon them, certain modifications of a less radical kind should also be alluded to. These minor modifications, so far as I have observed them, are of three principal kinds. First, we have those well-known changes by which a brownish so-called 'winter coat' is formed, and from the opening in

[1] He also states that similar living forms may be derived from the products of the repeated subdivision of Euglenæ as well as of Chlamydomonas. For reference to such a mode of origin of Monads from Euglenæ, see Dr. Gros' Memoir, p. 315; for Algæ, pp. 309, 322, 327; Pediastreæ and Desmids, pp. 303, 309, 318; and Diatoms, pp. 302, 309, 315. Again, with reference to the other organisms, Dr. Gros says, p. 455:—'Les Chlamidomonas à la 3e parifissure, convertissent aussi leurs 8 divisions en Clostériens (Pl. O. fig. 25) très agiles. . . . Les Chlamidomonas enfin peuvent se diviser énormément (fig. 24) et donner des Navicules et des Conferves.'

which two flagella are protruded[1]. Secondly, others develop a brownish and slightly indurated envelope, marked by dotted lines disposed in a spiral manner[2]; these forms being pointed at both extremities, motionless, and without flagella. And thirdly, others, also motionless, assume a spheroidal or beautifully ovoidal form, whilst their diaphanous and indurated testa becomes faintly striated after the fashion of a Diatom. The chlorophyll corpuscles contained in these forms also fuse into two or three large central masses of a very bright green colour[3].

7. *Transformation into Diatoms.* After Euglenæ have undergone two or three processes of fission—but most frequently after the third—some of them are apt at certain times to become converted into large Diatoms. Dr. Gros states that similar transformations may also be observed in specimens of Chlamydomonas, and that, in each case, the precise pattern of the Diatom varies according to the size and the nature of the contents of the vesicle which becomes transformed. He adds (p. 302):—'Si les vésicules sont fortes, bien vésiculés, nanties d'une certaine masse, on en voit dériver des Navicules striées. Ailleurs, on en voit dériver des millions

[1] See Gros, loc. cit., Pl. E, figs. 2, 12, 14, 15, 17.

[2] See Gros, loc. cit., Pl. D, fig. 3. The actual arrangement of the spiral lines was different in different specimens. Sometimes, as in the figure above referred to, the lines were equidistant, but at other times they were arranged in sets of twos or threes, with broader intervals between them.

[3] Such bodies very closely resemble fig. 6 of Pl. E in Dr. Gros' Memoir.

d'autres (Pl. L, fig. 6) dont la carapace est moins organisée et moins minérale[1].' Respecting the mode in which the conversion of the recently-divided Euglenæ takes place, Dr. Gros says[2]:—'Les vésicules derivées, par des circonstances imprescriptibles, de vertes et nucléolées qu'elles étaient, deviennent jaunâtres et diriment leur contenu en huit vésicules (Pl. F, fig. 2) sur la paroi intérieure[3]; elles s'allongent par des nuances de formes variées; et par un élaboration de quelques jours, elles arrivent (fig. 3, 4, 5) à donner ces physiognomies si jolies de Navicules striées dont on a fait tant d'espèces systématiques. Encore une fois, les vésicules derivées de telle forme et de telle division euglénienne, donneront, suivant la saison, des Navicules différentes.' And in illustration of this, he affirms that one set of Euglenæ produced *Navicula fulva* and another allied form; whilst a different set became transformed into *N. Margarifera* during the months of July, August, and September, although they no longer underwent such transformations in the months of December and January. Whilst I have not myself been fortunate enough to trace the actual origin of any of these large Diatoms, I have, on several occasions, been struck with the comparatively sudden appearance of very large specimens (about $\frac{1}{300}''$ in length) of *Navicula librilis*—

[1] He also adds:—'Vouloir essayer de représenter toutes les formes serait vouloir donner l'image des grains de sable de l'océan. Ici comme ailleurs, l'important est de donner la filiation et non la pittoresque des formes.'

[2] Loc. cit., p. 336.

[3] See Fig. 85, *a*, *b*, *e*.

still presenting an embryonic appearance—in vessels containing Euglenæ and Vaucheria.

8. *Transformation into Desmids and Pediastreæ.* The only Desmids that have been ascertained to be produced by the transformation of entire Euglenæ, are those large specimens belonging to the genus Closterium. Although I have not myself had the satisfaction of witnessing this transformation, Dr. Gros states that he has observed it on several occasions. The particular modification of the Euglena which is occasionally apt to undergo this change is, however, quite familiar to me. Specimens are frequently to be observed which, having lost their flagellum, are prone to assume an elongated worm-like form. They crawl, too, in a slow worm-like manner, rather than swim; and are always noticeable on account of the extreme brilliancy of the well-formed green vesicles which they contain, and of the bright carmine colour of their so-called 'eye-speck.' After a time, their movements grow more and more languid, and the green vesicles separate from one another at the middle of the body (*e*), so as to leave a clear space similar to that which also exists to a certain extent in the various forms of Closteria—into one or other of which these languid and elongated Euglenæ may, according to Dr. Gros, be gradually transformed. The transformation sometimes takes place in a few days, and sometimes only after two or three weeks; whilst other specimens of the same kind of Euglenæ may remain, even for months, without undergoing any noticeable alteration[1].

[1] See loc cit., p. 317.

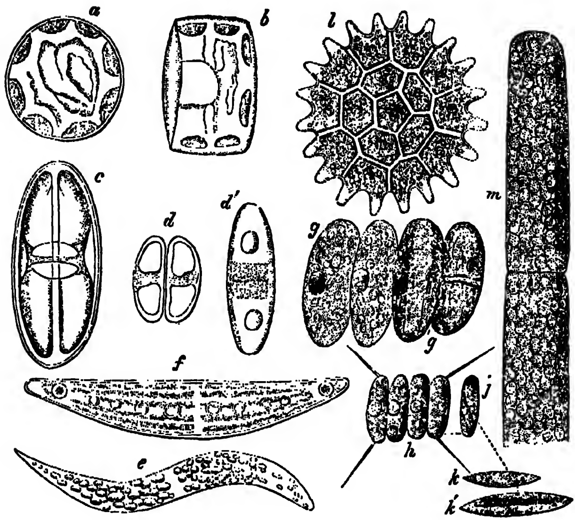


FIG. 85.

Origin of Diatoms, Desmids, Pediastreæ, and Algæ from Euglenæ and other Vegetal Matrices. (Gros.)

a. Euglena in early stage of transformation into a Diatom.
b, c. Two forms of Diatoms which may arise from transformed Euglenæ.
d, d'. Chlamidomonas giving origin to Diatoms.
e. One of worm-like Euglenæ, which after increasing in size may gradually become converted into a Closterium (*f*). (Reduced.)
g, g', A Euglena undergoes fission, and grows after the manner of Arthrodesmus.
h. A vegetal vesicle of Moss origin, which divides and develops into another form of Arthrodesmus.
j. One of its separate segments, dividing obliquely into two portions, each of which gradually grows and assumes the characters of a Navicula (*k, k'*).
l. A Micrasterias produced by the fission of a Euglena and the arrangement of the cohering segments in a single plane. (Reduced.)
m. Cladophora-like Alga produced from a Euglena (original x 600).

Although I have never seen the final stages of this transformation, I had, even before becoming aware of Dr. Gros' views, noticed the curious fact that very small specimens of Closteria were never to be seen. Wherever they are encountered one may see specimens of different sizes and of different patterns, though—with the exception of those which, from their want of symmetry, are obviously the products of a recent fission—they are all large and more or less full-grown. So that, just as in the case of the large Diatoms already alluded to, their origin by metamorphosis is much more reconcilable with these facts than with the notion that they are derived from small germs—more especially since no one has ever seen or knows anything about the mode of production of such germs in Closterium[1]. Of course we are far from implying that Closteria are only produced from Euglenæ; since what is known concerning the different modes of origin of other organisms might lead us to expect that Closteria would also be derivable from the transformation of other matrices, more or less analogous to Euglenæ.

Again, whilst the products of the third and subsequent fissions of certain Euglenæ occasionally become converted, in the manner described, into Diatoms, at other times such products may be transformed into Pediastreæ belonging to the genera Micrasterias or Arthrodesmus. Concerning the first kind of transformation (*l*), Dr. Gros says:—'Lorsque l'utricule euglénien conservant ses

[1] See Pritchard's 'Infusoria,' 4th Ed., p. 12.

vésicules parifissées adhérentes entre elles, va jusqu'à la 3e, 4e, 5e, 6e division ces vésicules s'arrangent sur un même plan; celles du contour poussent des cornes et présentent les jolies formes des Micrastérias [1].' Whilst the transformation into one or other of the varieties of Arthrodesmus (*g*, *h*) seems to occur still more frequently. Dr. Gros writes:—'Les Arthrodesmus qui ne sont que le 3e degré de parifissure prenaient des formes d'autant plus exigues que les utricules, d'où ils descendait, étaient plus petits [2].' One of the conditions under which Euglenæ are prone to undergo transformation into Pediastreæ has also been definitely ascertained. Dr. Gros observed that when Euglenæ were sown upon a small patch of damp earth some of them generally underwent this kind of metamorphosis, although others passed through different changes, so as to become converted either into Diatoms or into the organisms of which we are now about to speak [3].

9. *Transformation into Confervæ.* Not only may the ultimate products of repeated fissions of Euglenæ become converted into small Confervæ, as we have already stated (p. 443, note 1); but occasionally an Euglena, without such preliminary processes of fission, begins to vegetate so as to produce a much larger Algal filament

[1] See loc. cit., p. 311, and Pl. K, fig. 25.

[2] Pl. P, fig. 20–23. These transformations of Euglenæ into different kinds of Pediastreæ are also referred to by Dr. Gros at pp. 303, 309, 318, and 452.

[3] See p. 453.

of the Cladophora or Vaucheria type. This I have seen myself on several occasions, and especially amongst one set of Euglenæ which were left partially exposed to the air on some dead leaves. Some medium-sized specimens assumed a spheroidal shape, whilst their corpuscles became distinct and of a very bright-green colour. At the same time the red spot disappeared and the investing membrane became thickened. Some of these vesicles gradually elongated into filaments almost as broad as their matrices, across which dissepiments were formed at intervals (*m*). The chlorophyll corpuscles in the filaments continued to be of the same bright-green colour as they were in the vesicle from which they had proceeded; and for a short distance from their origin some of the filaments were invested by a thin sheath-like material, similar to what had previously constituted a kind of cyst for the metamorphosing Euglena. Other specimens of the same batch of Euglenæ were placed beneath a covering glass and kept within a damp chamber for two or three days, when some of them were found to have assumed the appearance and languid movements of the worm-like Euglenæ to which I have already referred (Fig. 85, *e*). The corpuscles in some of them became much elongated and the red speck disappeared. The organisms then became motionless, and, instead of transforming into Closteria, grew into narrow filaments of uniform diameter—in which the corpuscles were rather sparsely distributed, although they continued to have the same elongated appearance as they had in

their Euglenian matrices. The filaments themselves, but for their being much more slender than usual, resembled and grew after the fashion of Vaucheria[1].

Dr. Gros also speaks of the transformation of specimens of these worm-like Euglenæ into Confervæ. Some of them produced Closteria and various animal forms in the months of August and September, though others did not become transformed till November and December. Concerning these Dr. Gros says[2]:—'Des grandes Euglènes donc (Pl. L, fig. 11-14) ont pris de la nourriture et de la vésiculation (fig. 12) traînent une vie languissante et se transformant en une tronc (fig. 11) Confervien qui se constitue une Conferve (fig. 13), susceptible de se développer ultérieurement, comme nous l'avons déjà vu pour d'autres espèces.' And in reference to another stock of Euglenæ, some of which had also given origin to Desmids, Dr. Gros says[3]:—'D'autres vésicules Eugléniennes prennent une forme végétative Confervienne plus claire, et ces végétations deviennent assez abondantes pour augmenter la teinte verte de l'eau.'

We have elsewhere[4] given an abstract of the evi-

[1] In a road-side ditch at Hendon, from which I frequently procured supplies of Euglenæ, I found on several occasions, during the months of January and February, that when the quantity of water became diminished so as to leave the Euglenæ just above the water-mark, beautiful patches of Vaucheria speedily appeared in these situations. At other times Oscillatoriæ have been seen to develop in abundance under similar conditions.

[2] Loc. cit., pp. 338 and 318.

[3] Loc. cit., p. 302.

[4] *Appendix* D, pp. lix-lxiii.

dence adduced by Dr. Braxton Hicks as to the extreme modifiability of the simplest forms of Algæ, and also as to the relationship which he, Itzigsohn, and others have shown to exist between these forms of life and Lichens. This and much other information tends to show that Lichens and Algæ are mere different modes of growth which may be assumed by one and the same matter when it undergoes internal changes—either 'spontaneously' or in response to alterations in external conditions[1]. It was also ascertained by the same observers that green elements thrown off from the radicles or leaves of mosses might live and vegetate for an indefinite period, after the manner of one or other of the Algæ, and that then, after a time, many of such forms might (under suitable conditions) develop 'soridia,' constituting the commencement of a new phase of growth, which gradually unfolds into one or other of the common Lichens[2]. But whilst it has been long known that Mosses were constantly developed from similar confervoid modes of growth, it had not been thoroughly established that they might arise from Confervæ which

[1] See p. 164. Quite recently I have seen in a vessel containing an old and partly-decolourized Euglena-pellicle, the whole upper surface become, almost simultaneously, covered with a dry pulverulent growth of Chlorococcus, from which Lichens are so apt to develop. The pellicle was thick, and its upper surface dry, whilst for three weeks before the appearance of the Chlorococcus the vessel containing it had been covered with a bell-jar.

[2] These views are also supported by the observations of Mr. Metcalfe Johnson. (See 'Monthly Microsc. Journ.' of Nov. 1871.)

were not themselves the direct descendants of Mosses. Many facts, however, which have been made known, both before and since the date of these observations, seem to favour the possibility of the occurrence of such a metamorphosis. It has been affirmed to take place, for instance, by Prof. Schaaffhausen[1], although more positive information to the same effect had long previously been supplied by Dr. Gros. The latter says[2]:—'Des essais fait avec soin prouvent que l'on peut semer des animaux et récolter des plantes. En effet, de la marne, prise à 20 pieds de profondeur, fut ensemencée d'Euglènes et recouverte d'un disque de verre. Les Euglènes se mirent à se parifisser, et donnèrent les unes des animalcules qui moururent, les autres des cellules qui se convertirent en Navicules, les troisièmes donnèrent des cellules qui se mirent à végéter, non seulement comme les Conferves aquatiques, mais comme des Mousses aéricoles qui atteignait 13 millimètres de hauteur à la fin des expériences. La parifissure, le commencement de végétation, la multiplication des cellules végétales avaient été constatés tous les jours avec le microscope.' And if doubts may be entertained with regard to the conclusiveness of these observations, owing to the possibility of the chance introduction of a few real Moss-germs, which during their germination were not discriminated from the fissiparously-produced descendants of Euglenæ—such doubts are wholly inadmissible

1 'Cosmos,' 1863, t. xxii. p. 631.

2 'Ann. des Sc. Nat.' 1852 (Zool.), p. 201.

with regard to the following case, cited by the author of 'The Vestiges of Creation.' He says[1]:—'In a work upon the useful Mosses, M. de Brébisson states that a pond in the neighbourhood of Falain, having been rendered dry during many weeks in the height of summer, the ground was immediately and entirely covered, to the extent of many square yards, by a minute, compact green turf, formed of an imperceptible[2] moss, the *Phaseum axillare*, the stalks of which were so close to each other that upon a square inch of this new soil might be counted more than five thousand individuals of this new plant, which had never previously been observed in the country.' The simultaneous growth in one small spot of hundreds of thousands of specimens of this particular Moss might be easily reconcilable with their heterogenetic origin from Confervæ or algoid vesicles of some kind[3]; whilst an explanation of the phenomenon on any other hypothesis would seem to be absolutely irreconcilable with all known facts concerning the growth of Mosses and concerning the comparative paucity with which the reproductive ele-

[1] Tenth edition, 1852, p. 201.

[2] Not actually 'imperceptible,' of course, although the several plants might have been more or less indistinguishable from one another in the green turf formed by their aggregation.

[3] Dr. Gros says:—'Les Conferves les plus diverses peuvent descendre d'une même semence, selon le degré de division et les circonstances de développement. Cette semence change de qualités, par un travail intérieur mystérieux. La végétation confervienne qui dans les eaux, en reste aux formes cellulaires aboutées, se complique et se cellulise, et donnent des mousses dans un milieu aérien.'

ments, even of the commonest of such Cryptogams, are to be found in the atmosphere.

It seems, however, to be quite certain that a community of nature exists between Algæ, Pediastreæ, Desmids, and Diatoms, since similar vegetal cells may, on the same or on different occasions, grow into forms belonging to either one of these groups; and, moreover, the forms are strictly convertible with one another until they chance to assume the forms of Diatoms. This latter step in molecular composition, when once it has been entered upon, cannot be retraced. Diatoms constitute the terminal forms of a divergent series. The middle terms of the series, however, viz. Pediastreæ and Desmids, are convertible in both directions, either back into Confervæ or onwards into the less-vitalized Diatoms. Thus, after having spoken of the latter transformations, to which we have already referred, Dr. Gros says:—'Il peut se faire aussi que les frustules d'Arthrodesmus et de Micrastérias, en tombant sur un sol humide seulement, tournent à la vie végétale, sois qu'ils dérivent de Mousses, d'Euglènes, ou de Chlamidomonas.' Whilst elsewhere the same observer speaks of specimens of Arthrodesmus which subsequently gave birth to unmistakeable Confervæ[1].

Having considered these transformations of Euglenæ

[1] See loc. cit., pp. 452 and 333. Some of the Pediastreæ found in my experimental flasks were also seen to grow after the manner of a Conferva (see vol. i. p. 453).

into organisms of a more or less vegetal type, we have now to refer to their metamorphoses into decidedly animal forms of various grades of organization. Nothing is more startling, and yet nothing more common, than to see neighbouring specimens of the same stock of Euglenæ, without any appreciable cause, turning along totally different lines of development. As Dr. Gros pointed out, 'suivant des circonstances souvent inappréciables, on voit une vésicule suivre un développement animal, tandis que sa congénère et jumelle suit un rhythme végétal.' He also adds:—'Les circonstances de chaleur, de saison, de lumière, de quantité et de qualité de matière, le plus souvent imponderables, donnent lieu à des caprices de reproduction, si l'on osait appeller caprices ce qui ne tient, qu'à l'insufficance de nos moyens d'observation.'

10. *Transformation into Actinophrys or Amœbæ, which subsequently become converted into various forms of Ciliated Infusoria.* The Actinophrys and the Amœba are regarded by Dr. Gros[1] as mere intermediate modes of existence into which Euglenæ are apt to lapse when the conditions operating upon them are not favourable to their more direct transformation into higher forms.

I have several times had the opportunity of watching the different stages through which Euglenæ pass during their transformation into Amœbæ. It most frequently occurred in this manner:—The Euglena became motionless and somewhat irregular in shape, whilst its chloro-

[1] See loc. cit., p. 330.

phyll vesicles enlarged and assumed a very bright-green colour. Its outer surface underwent no condensation. On the contrary, it seemed gradually to become more plastic, whilst it also became decolourized, and studded with a number of small ovoid, colourless, and refractive particles. The large bright-green chlorophyll vesicles had by this time become closely aggregated and even partially fused into one mass, which slowly underwent decolourization from periphery to centre. The molecular changes going on in the superficial colourless portions seemed to be capable of effecting the direct transformation of the chlorophyll vesicles into colourless chlorophyll, since the central mass gradually became smaller and smaller, without any of the usual intermediate shades of colour revealing themselves. And by the time the green mass had half disappeared, the colourless peripheral portions of the transforming organism were exhibiting distinct amœboid contractions and alterations in shape. The ovoid refractive particles also soon began to disappear, so that when the central portions of chlorophyll had been completely decolourized, the mass was converted into a rather sluggish, finely-granular Amœba, which developed vacuoles in its interior, became more and more active, and at the same time began to take food into its substance and increase in size in the ordinary manner[1].

[1] Dr. Gros says:—'Chaque vésicule, ici comme ailleurs qui est destinée à reproduire un Plæsconien ou Oxytriqué, et qui n'a pas encore assez de matière en soi, est comme un œuf, qui passe par la forme amœ-

At other times—and in Euglenæ which had become spherical, although not encysted—I have seen the transformation take place after a different fashion. The chlorophyll vesicles broke up so as to resolve themselves into green granules—which speedily assumed different shades of colour (such as olive, brown, and yellow) before complete decolourization. Some highly characteristic specimens were seen, in which the red spot still partly remained, and in which the majority of the granules were of a greenish colour. But in other contiguous vesicles of the same size nothing but granules were to be seen, partly colourless and partly of an olive and brownish yellow colour. Gradually all the granules became decolourized, and the substance of the organism having become more fluent exhibited slow amœboid alterations in shape. And in proportion as the large granules disappeared, so did the mass become more and more active, till at last it was converted into an ordinary finely-granular Amœba.

The conversion of Euglenæ into Actinophrys I have not seen, though it seems to have been frequently observed by Dr. Gros. The Euglena whilst still in its green state protrudes ray-like projections from its surface, and gradually undergoes an internal elaboration and molecular transformation, in the progress of which it becomes decolourized, and at the same time more

béenne, jusqu'à ce qu'elle ait sa quote-part de substance nécessaire à ses métamorphoses ultérieures.' (Loc. cit., p. 311. This transformation is also mentioned on pp. 305, 314, and 318.)

active and more thoroughly animalized. In this form it takes food into its substance, assimilates it, and undergoes a certain increase in size; till at last it again becomes sluggish, assumes a spherical or ovoidal form, and retracts its pseudopodiæ one by one preparatory to new transformations [1].

At other times, according to Dr. Gros, a decolourized and animalized Euglena may assume for a period the form of a Perancma before becoming converted into an Amœba or an Actinophrys [2], and these latter forms, when they have acquired a certain (though unknown) stage of molecular elaboration, tend to become converted into different forms of Ciliated Infusoria [3].

11. ***Direct Transformation into one or other of the Ciliated Infusoria.*** But at other times the transformation of the Euglena takes place in a different manner, so that, as Dr. Gros pointed out, it is enabled at once to acquire the requisite molecular composition, and pass to the form of a Vorticella or an Oxytricha— without previously existing in either of the abovementioned intermediate and less specialized states. Speaking of some Euglenæ which underwent this metamorphosis, Dr. Gros says:—'Quand elles se transforment de toutes pièces ou après la première parifissure elles suivent une rhythme générale, que nous retrouvons ailleurs sur un plus grande échelle. Elles prennent une

[1] See loc. cit., pp. 318, 335, and 336.
[2] See loc. cit., p. 336.
[3] See loc. cit., pp. 305, 314, 318, 335, 336, 435.

forme plus ou moins sphérique, et offrent *constamment* une décoloration de leurs vésicules internes qui passent du vert au jaune, à l'orangé, au rouge plus ou moins foncé, au brun-noirâtre, qui palit peu à peu (Pl. D, fig. 5–9) et le résultat final de cet métamorphose est la conversion de la membrane euglénienne et de son contenu en une membrane de Vorticelle, de Plæsconien, de Kéronien[1].'

These transformations of Euglenæ into Ciliated Infusoria, take place either immediately or only after a previous period of encystment.

I have seen evidences of the immediate transformation on several occasions in which the phenomena were of such a character as to preclude all possibility of errors of interpretation. Thus, motionless ovoidal bodies were seen, each about the size of an ordinary Euglena, and in a state of partial transformation, but presenting no trace of the existence of anything like a cyst. More than half the mass had perhaps been decolourized, though a small portion of the red 'eye-speck'

[1] Loc. cit., p. 298. Dr. Gros also says:—'La bizarrerie de ces faits s'affaiblira dans l'histoire des phases elles-mêmes. C'est pourquoi il est prudent de ne pas se prononcer trop tôt quand on a sous les yeux des exemples de transformations, exemples qui n'excluent pas les autres transformations possibles dans d'autres conditions. Les expériences faites à la maison, dans quelques vases, livrent bien des faits, mais elles n'épuisent pas la latitude de la loi.' And elsewhere (p. 204), speaking in a similar strain, he says:—'Or, pour avoir observé les transformations de ces petits êtres dans un direction et de certains circonstances, on n'est nullement autorisé à nier ou rejeter la possibilité d'autres évolutions. La loi c'est la variété!'

might still remain, and also, an aggregation of partly greenish and partly brown granular matter in the more central parts of the animalizing mass, representing the as yet unmetamorphosed portion of the green contents of the Euglena (Fig. 86, *a*). Cilia are not usually protruded at such an early stage as this, although on one occasion in which the transformation was scarcely so advanced, almost motionless cilia were seen to exist. This was in an ovoid transforming Euglena which still contained a number of minute green corpuscles, mixed with granular matter in different stages of decolorization, and also a large colourless nuclear corpuscle at one extremity (*b*). The whole body was motionless, though it was uniformly fringed with short and very languidly moving cilia, which had all the appearance of having been recently protruded. Cilia first appear, as I have frequently observed, in the form of minute motionless protrusions, which gradually elongate and soon begin to exhibit very slow vibrations. In the course of from fifteen to twenty minutes they may be observed to have attained a medium length, whilst they exhibit languid but regular movements. The cilia are protruded after precisely the same fashion as the rays of an Actinophrys, and these latter are also, like the cilia, almost always motionless at first[1].

On other occasions the Euglena undergoes a complete

[1] In the origin of the Amœba itself the same sort of progression is noticed. The colourless protoplasm, when it begins to move, moves only very slowly, and it very gradually acquires an increased mobility.

decolourization, and becomes converted into a finely-granular spherical or ovoidal mass before any cilia are protruded. Such bodies, devoid of cilia, may occasionally be seen lying side by side with others, in which short cilia exist—either wholly motionless or with a few of them exhibiting slight flickering movements (Fig. 86, *c*). Some of these were embryos of unknown forms of Ciliata; though others, judging from the disposition of their setæ, seemed undoubtedly to be embryo forms of Oxytricha[1]. On another occasion the decolourized spheres became hemispherical, and protruded stout setæ from the under surface, which soon began to exhibit slow movements after the fashion of Trichoda. Dr. Gros also expressly states[2] that he has seen Euglenæ become decolourized without previous encystment, develop ciliæ, and take on the very special characters of Coleps; whilst elsewhere[3] he seems to imply that Vorticella, Nassula, Oxytricha, and Enchelys may be produced in a similarly simple manner from transforming Euglenæ. And yet, with reference to each of these forms, he is also careful to add that their appearance upon the scene may be the result of transformations taking place in quite different matrices.

So far as I have at present observed, the majority of those Euglenæ which become encysted at or before the period of transformation, are converted into spherical

[1] Very similar to that of Fig. 90, *f*.
[2] Loc. cit., p. 314, Pl. E, fig. 27 *a*, *b*, *c*.
[3] Loc. cit., pp. 306, 312, 336, Pl. E, fig. 23-36.

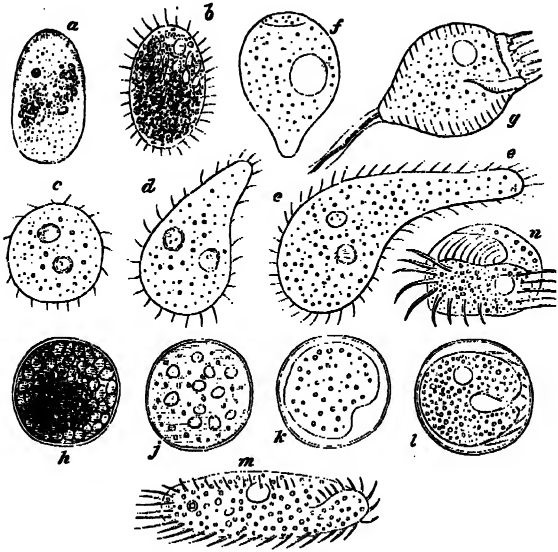


FIG. 86.

Modes of Origin and Development of Ciliated Infusoria. (× 600.)

a. A transforming Euglena, with red 'eye-speck' still visible.

b. A similar body, having many of its chlorophyll corpuscles still green, fringed with almost motionless cilia.

c. A completely decolourized sphere derived from a transformed Euglena, provided with a few partly-motionless cilia.

d and *e*. More advanced forms of a similar embryo developing into a Dileptus (?).

f. Vorticella, soon after its emergence from a cyst of Euglena origin, which subsequently develops into a striated variety (*g*).

h. A large Chlorococcus-vesicle, whose contents gradually undergoes decolourization (*j*), and at last becomes converted into an animalized mass (*k*), which gradually shapes itself into the form of an Oxytricha (*l*). This after a time ruptures its cyst and soon takes on the characteristics shown at *m*.

n. A form of Plæsconia derived from an embryo produced within other, apparently similar, Chlorococcus-vesicles.

Vorticella embryos[1]. The change generally takes place in specimens which are already encysted, and which are lying side by side in a kind of tesselated pellicle formed by closely-packed Euglenæ. The early stages of the transformation are precisely similar to those which have been hitherto described. Decolourization is gradually completed, till at last a whitish and very finely-granular mass is produced, spherical in shape, and enclosed within a rather thick cyst-wall. These bodies vary in size according to the dimensions of the Euglenæ which undergo transformation, and those which I have mentioned ranged from $\frac{1}{1000}''$ to $\frac{1}{500}''$ in diameter. They replace the Euglenæ, so that they remain as integral though metamorphosed parts of the coherent tesselated layer. Very soon a vacuole makes its appearance near the centre of the embryo, which subsequently remains—disappearing only at short intervals. These embryos, unlike those of Paramecium, do not rotate within their cysts, and do not seem to exhibit any movements until they are about to become free. In what precise manner they effect their exit I have never been able to ascertain, though I have several times seen an embryo very shortly after its emergence from its cyst (*g*). At this period they form ellipsoidal masses of finely-granulated protoplasm, generally containing one large vacuole, and presenting

[1] Dr. Gros also distinctly states (p. 312) that Vorticellæ may arise in this manner—though he makes no express statement concerning a similar origin for other forms.

obscure evidences of circular striation—whilst at one extremity (posterior) there is a more transparent conical projection. The embryo remains almost motionless, except that about every half-minute a sudden contraction, with invagination of the posterior part of the body, takes place. In the course of a few minutes an eversion of the anterior portion of the organism occurs, so as to form a sort of collar-like rim, from which a row of about 8–12 stout cilia begin to protrude. These cilia are motionless at first, but they have been seen to begin to play in from fifteen to twenty minutes. Previous to this, however, the sudden telescopic contractions had been affecting the anterior part of the body as well as the posterior, so as on each occasion to produce an infolding of the ciliary wreath. By this time also slow movements of the contained granules were seen, whilst two or three vacuoles frequently appeared and disappeared. After thirty minutes the organism had often assumed an obovoid form, and become distinctly striated, whilst its ciliary wreath might be seen in full play. It soon anchors itself also by its posterior sucker-like extremity, and continues to exhibit sudden contractions at intervals of a minute or less. The continuance of these sudden contractions whilst the organism is in a fixed position, very soon suffices to produce a pedicle, which pretty rapidly elongates. A specimen in which the pedicle was just about to form was kept under observation for thirty minutes, and in this time it was found to have attained a length

of $\frac{1}{1428}''$, that is to say, it had grown to one-third of the length of the body of the organism. Gradually a lateral extension of the oral cleft forms, and becomes lined with cilia so as to perfect a form of Vorticella (Fig. 86, *g*) similar to that which I have generally seen proceed from the metamorphosis of an Euglena.

Other transformations of Euglenæ have been described by Dr. Gros of a still more startling nature, the consideration of which, however, it will be better for us to defer for the present, until we have enquired more fully into the evidence bearing upon the modes of origin and life-history of the Ciliated Infusoria. But in order to complete our list of the known changes which Euglenæ may undergo, it will be desirable simply to name the metamorphoses of these protean forms which still remain to be considered. They are:—

12. *Transformation into Rotifers;*
13. *Transformation into Tardigrades;* and
14. *Transformation into Nematoids.*

Remarks upon the various Modes of Origin of Ciliated Infusoria.

It has already been stated concerning almost all the forms of Ciliated Infusoria which I have had occasion to mention, that they may proceed, on different occasions, from apparently dissimilar matrices; and also that their mode of development is subject to much

variation[1]. It will be well, however, to show a little more fully that Ciliated Infusoria agree in both these respects with what has been already established[2] for Fungus-germs, Monads, Amœbæ, and other closely related forms of life—of which, indeed, the former are only more highly developed representatives.

Much evidence exists tending to show that green vegetal vesicles, whether derived from Mosses or from any of the multitudinous forms of Algæ, may at times undergo transformative changes closely corresponding to those which Euglenæ of a similar size are apt to pass through. Thus, although I have never seen encysted specimens of the latter organisms converted into Oxytricha or Plæsconia, I have many times seen both these forms of Ciliata arise from large vesicles of Chlorococcus. This form of Alga generally consists of rather small corpuscles (from $\frac{1}{7000}''$ to $\frac{1}{5000}''$ in diameter) which multiply in a pellucid jelly; but, especially when growing near the surface of the water, some of its vesicles are very prone to continue increasing in size, owing to a cessation of the process of fission. They thus give rise to separate vesicles varying in size from $\frac{1}{5000}''$ to $\frac{1}{550}''$ in diameter, and composed of small, bright-green, chlorophyll corpuscles densely packed within

[1] As a rule, it may be said that those which arise from an encysted mass of transforming matter begin their existence with more perfect forms than those which proceed from the molecular transformations of non-encysted masses of protoplasm.

[2] In Chaps. xvii. and xx.

a colourless but thick cyst-like envelope (Fig. 86, *b*). Very many of these bodies seemed to remain stationary when they had attained the size of $\frac{1}{1000}''$ in diameter;* and some of them might be seen whose contents were undergoing various stages of decolourization, whilst in others, lying by their side, all the colouring matter had disappeared and was replaced by a mass of structureless protoplasm containing a few granules of different sizes (*j*, *k*). These masses of protoplasm gradually underwent a series of molecular changes, during which old granules disappeared and new granules, of a rather large size, took their place. The mass then began to shape itself, whilst cilia developed at each extremity, by means of which it commenced rotating irregularly within its cyst, the walls of which had now become much thinner (*l*). The form of an Oxytricha was, at this stage, distinctly recognizable within the cyst, which after a time gave way and liberated an organism $\frac{1}{550}''$ in length, containing dirty-looking granules similar to those of the specimens of Oxytricha already existing in the water (*m*). Although organisms of this kind were produced from the majority of the vesicles, in others, which appeared in every way similar, the embryo mass, owing to some unknown cause, was seen to shape itself into the form of a Plæsconia having four or five very deep and longitudinal dorsal depressions. These embryos ultimately moved about with extreme activity within their cysts. Occasionally also another form of Plæsconia was produced whose dorsal shield was slightly convex and

almost smooth, whilst its under surface was much more complex (*n*). Such specimens of Oxytricha and Plæsconia were, moreover, the only forms of Ciliata seen in the solution from which the Chlorococcus vesicles were taken. The origin of Oxytricha within the filaments of Nitella has also been already referred to[1].

Facts just as remarkable can be stated concerning the different modes of origin of Vorticellæ. Thus, although Vorticella-cysts are so frequently derived from encysted Euglenæ, I have seen algoid vesicles budded off from Vaucheria[2] (as well as others which have arisen from the very common but protean Alga named *Lyngbya muralis*[3]) also converted into Vorticella-cysts, and these producing organisms in almost all respects similar to those from cysts of Euglena origin. According to Dr. Gros[4], moreover, cellular bodies budded off from Moss-sporangia may also undergo transformative changes in all respects similar to those of Euglenæ.

In many other cases, however, Vorticellæ seem to arise in an altogether different manner. Instead of being produced by the molecular transformation of masses of matter which are at once converted into full-sized though embryonic individuals, they are derived from vesicles containing an animalized matter, which bud out from, or are protruded by, certain vegetal cells

[1] See p. 404.
[2] See Fig. 82, *f*, and p. 415.
[3] See *Appendix* D, p. lx.
[4] Loc. cit., pp. 448, 487.

or filaments[1]. This mode of origin was distinctly indicated by Dr. Gros in reference to the changes that might occur in the leaves of aquatic plants. He said[2]:—'Les cellules des feuilles laissent leur vésiculines chlorophylliennes se faner, ou bien elles poussent un utricule hilé qui s'élabore en Vorticelles, de forme diverse, selon la quantité de matière et la qualité des vésicules.' Repeated observations on Vaucheria, as well as other Algæ and aquatic plants, have led me to believe that this is one of the most frequent modes of origin of the Vorticellæ which are constantly found upon their surface; whilst, according to M. Nicolet[3], Vorticellæ may be produced upon the filaments of Nitella in an almost similar manner. On other occasions Vorticellæ have been seen to develop from an internal bud (which appears after the manner of a nucleus) within the clear anterior extremity of Chlamydococcus corpuscles. This mode of origin has been seen by Mr. T. C. Hildgard, who says:—'This parasite is a perfectly colourless globule, apparent in the clear navel-point of the cell, and exhibits a faintly opalescent hue. As it grows, the cell which harbours the "incubus" loses its own individual vitality.' Its external coat hardens, but its internal contents gradually disappear, as the embryo 'grows and occupies more space, executing tremulous

[1] Just as in other cases vesicles are produced which become converted into Desmids or Diatoms, or whose contents become resolved into algoid elements (see pp. 418, 419).

[2] Loc. cit., p. 448.

[3] See p. 478.

and vibratory contractions.' After a time the cyst is ruptured, and then the granular embryo 'after a few very wry contractions, at once widely opens a large, ciliate mouth, gaping across the sphere's surface; and disengaging or displaying a girdle of cilia round the rear part of the body, it immediately represents the free-roving Vorticella in full equipment[1].' M. Pouchet, moreover, depicts[2] vesicles gradually increasing in size, which ultimately became converted into Vorticellæ; and lastly, M. Pineau described the mode of origin of Vorticellæ from vesicles which had been developed in the pellicle by a process of synthetic heterogenesis, similar to that which gives birth to Paramecia and Kolpodæ. These vesicles, after increasing in size, first assumed the form of Actinophrys, then of Acinetæ, and ultimately became converted into well-developed Vorticellæ[3].

In the last-mentioned mode of origin of Vorticellæ, the starting-points were certain vesicles or corpuscles developed from the pellicle, by a process similar to that whereby Monads and Amœbæ have been shown to arise both in the pellicle and in other organic aggregates[4]. It is now, therefore, of importance to be able

[1] See 'Monthly Journ. of Microsc. Sc.,' Nov. 1871, p. 229; and Silliman's 'American Journ.,' Aug. 1871.

[2] 'Hétérogénie,' Pl. I.

[3] See p. 252.

[4] It has, moreover, been fully proved that such mere motionless corpuscles, as well as Monads, Amœbæ, and Fungus-germs, are all interchangeable and convertible forms of living matter. (See Chap. xvii.)

to show that other forms of Ciliated Infusoria are also frequently produced by the further growth and development of Monads and Amœbæ.

It has been already stated that the embryonal spheres which are so abundantly produced within the filaments of a dying Nitella may segment into Flagellated Monads, or that they may be wholly transformed either into Amœbæ and Actinophrys, or into some one or other of the forms of Ciliated Infusoria [1]. I have, however, on several occasions been able to watch the stages by which these Flagellated Monads after increasing in size become converted into Amœbæ; whilst the latter, after undergoing some increase in bulk, become motionless and lapse into more or less ovoidal forms. The motionless bodies thus produced gradually protrude cilia from various parts of their surface, and are very similar to the smaller embryonal spheres of Nitella, which also develop at once into various forms of Ciliata [2]. In both cases the cilia that are at first protruded are motionless, and they subsequently move in a slow and languid manner, before vibrating with sufficient rapidity to produce active movements of the whole organism. The Monads developed from the external vesicles of transforming Euglenæ [3], and which have been shown to be almost interchangeable with Fungus-germs, are also frequently seen to undergo similar developmental changes. They increase in size, the nuclear body breaks up into many

[1] See pp. 402–424. [2] See Fig. 80, *c*, *c'*, *d*, *d'*. [3] See p. 437.

smaller portions (Fig. 84, *h*, *j*), and after having attained a length of $\frac{1}{1250}''$ they become almost motionless, lose their flagellum, and then develop either into small forms of Ciliated Infusoria, or else become converted into active Amœbæ (*k*, *l*, *m*). The latter, after increasing much in size, may—as Dr. Braxton Hicks[1] and Prof. Schaaffhausen[2] stated several years ago, and as Dr. Gros had long previously announced—ultimately become transformed, with or without previous encystment, into some larger forms of some of the Ciliated Infusoria[3].

Evidence of the most varied nature, indeed, as well as the independent testimony of many successive observers, all concur in pointing to the conclusion that the precise form of life produced in cases of heterogenetic transformation is to a very great extent dependent upon the size or mass of the matrix which undergoes transformative changes. This notion is impressed upon us by Dr. Gros in almost every page of his memoir; it was the view independently adopted by Mr. Carter[4]; and again, later still, in 1859, it was the doctrine announced by M. Nicolet—based upon obser-

[1] See p. 378. [2] Cosmos, t. xxii. p. 635.

[3] Mr. Metcalfe Johnson frequently speaks of the development of Paramecium and Kolpoda from Monad forms (see 'Month. Microsc. Journ.' Aug. 1869, Jan. 1870, and Nov. 1871, Pl. CIII. fig. vii.); whilst Prof. A. M. Edwards of New York has recently watched the conversion of Amœbæ into Ciliated Infusoria of the same kind ('Proceed. of Lyceum of Nat. Hist.' 1871, p. 216).

[4] 'Ann. of Nat. Hist.,' vol. xvi. Although subsequently, as we have already pointed out, Mr. Carter gave a different interpretation to the facts (see p. 391).

vations which apparently were made whilst he was in entire ignorance both of the facts and of the views announced by Dr. Gros and Mr. Carter[1].

But whilst the influence of actual mass is most important, I am fully convinced that the existence of a certain molecular composition has really more to do (as a determining cause) with the origin of higher forms than the mere bulk of the mass which undergoes transformation[2]. It is, however, quite true that, under the same conditions, similar matter will often transform itself into higher and higher forms (either directly or indirectly), according as the size of the mass which undergoes transformation increases.

This is well exemplified by the results of some observations recorded by M. Nicolet, in which the protoplasmic contents of one of the internodes of Chara may, he says, be seen to give birth to a teeming progeny of independent living things, which subse-

[1] M. Nicolet says:—'In organic chemistry the proportion of the atoms determines the substance, here the proportion of the granules seems to determine the species. This will explain some of the singular anomalies which are observed in the development of certain Infusoria, and the difference in the final form assumed according to the more or less abundant supply of nutriment.'

[2] A large mass of matter, under the influence of unsuitable conditions which suffice to alter its molecular constitution, may be compelled to assume the comparatively low mode of existence of an Actinophrys or Amœba; or it may be compelled to segment into Monads or Fungus-germs—even if, under still worse conditions, it does not become resolved into a swarm of Bacteria. On the other hand, the complex egg-like bodies produced within an internode of Nitella may, as we have seen (p. 406), be either large or small.

quently undergo the most startling series of developmental transformations.

In order to watch these changes, all that is necessary, according to M. Nicolet, is to prepare one of the internodes of Chara in the following manner:—Having stripped off the peripheral cells which form a kind of sheath for the central compartment, a fine thread is to be tied pretty tightly around each end, just within the node; the nodes themselves should then be cut off, and, after all foreign material has been removed from its surface by means of a soft brush, the portion of the internode between the two ligatures should be immersed in a vessel of very pure water and maintained there in a more or less vertical position. Thus prepared, the cell ought to be quite transparent, and, when examined with the microscope, the circulation of the cell contents should be easily observable. This circulation continues for a variable time—days, or even weeks—according to the degree of vitality of the plant to which it formerly belonged. After a time, however, certain other changes take place, which are thus described by Nicolet:—'In following the movements of the liquid, it may soon be noticed that it deposits, at the inferior extremity of the cell, a material which is more dense and more glutinous than that of the liquid in movement, and which soon begins to become rounded, turning on its axis, on account of the impulsion it receives from the movement of the fluid in which it is immersed, though it remains in the same situation. . . .

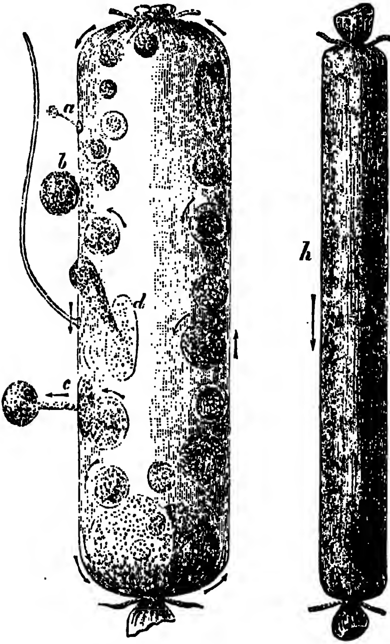


FIG. 87.

Development of Infusoria from the Protoplasm of Chara. (Reduced from Nicolet.)

Portions of two internodes of Chara prepared in the manner described; one (slightly magnified) as it appears when first prepared, and another (more highly magnified) representing a later stage, in which revolving spheres of various sizes are to be seen. Other masses are attached to the wall of the internode, and are already developing external vesicles (*a*, *b*, *c*, *d*), from which various forms of Infusoria are to be developed —*d* being the kind of matrix which becomes transformed into a Rotifer.

At the expiration of a day or two, one perceives that this matter has divided in order to form itself into

smaller globules, which, in their turn, subdivide into others smaller still. This division, on account of its necessarily rendering the different detached portions lighter, leads to their circulation. Each new globule, carried away by the general movement, describes an ellipse, whose length is inversely proportionate to the size of the corpuscle. But whilst the different corpuscles, which are natural to the liquid, follow the current without turning upon their axes, the others, by virtue of their primitive rotation—a movement which has not been destroyed by the process of segmentation—move under the double influence of rotation and of circulation. . . . As the process of segmentation advances, so does the nature of the matter constituting the globules seem to alter. The granulation, which was at first superficial and irregular, becomes internal and regular; the refraction, which at the commencement was inferior to that of water, becomes equal and then superior to it. In some globules this matter seems to isolate itself from the surface, and to form centrally a kind of nucleus, variable and irregular, but always having rounded angles, whilst the surface remaining, as it were, suspended, takes on the appearance of a thin layer. . . . If, in this stage, a cell is emptied upon a slip of glass, in order to examine its contents, it may be perceived that these latter corpuscles have become vesicles full of a glutinous liquid, colourless and very transparent, in the centre of which is a mass of denser material of a granulated texture, elastic, whitish, and

with the exception of mobility, possessing all the characters of that constituting an Amœba. By the side of these there are other masses without any apparent membranous envelope, whose substance is similarly glutinous and granulated; whilst elsewhere other globules are found, whose development is undoubtedly less advanced, showing a less refractive substance, and an irregular granulation which appears more on the surface than in the interior. . . . The quantity of this glutinous matter increases as the cell becomes older, and its different states necessarily indicate a slow modification of the nutritive juices of the plant—a modification which tends in a manner to animalize it, since from this very matter there arises, as we shall now see, a multitude of Infusorial animalcules. . . . In their course from one extremity to the other of the cell, the different globules, whose formation I have endeavoured to describe, finally attach themselves to the internal wall, and form upon different parts of it an irregular layer, which is transparent and more or less mammellated. It is then that the Infusoria make their appearance: Monads, Amœbæ, Keronæ, Vorticellæ, Actinophrys, Rotifers, all appear, all show themselves successively whilst passing through different stages of development, and in from fifteen to twenty-one days the vessel is crowded [1]. All of them commence by the vesicular pro-

[1] M. Nicolet says:—'A vessel prepared on the 29th of April, and containing a single cell of Chara, yielded by the 15th of May following, in addition to an incalculable number of [smaller] Infusoria, one hundred

jection similar to that which I have described in connection with Trichomonas[1]—a projection whose volume increases, in proportion as the substance on the corresponding part of the inner wall of the cell diminishes. All pass through numerous transformations before attaining their final form, though all do not attain this form. Some become the prey of other Infusoria; some, arrested in their development, owing to causes which remain hidden, return to their primitive form—that of the Amœba.'

Again, after what has already been stated, it becomes more easy for us to accept the fact that the Ciliated Infusorium named Otostoma by Mr. Carter, may arise within the closed internodes of Nitella, in the manner which he originally described.

The following observations were repeated by Mr. Carter on several occasions. He says:—'About three weeks after gathering plants of Nitella and placing them in a basin of water, the green layer of the long slender internodes becomes separated from the cell wall, and gathered up into dark, spherical bodies, averaging about the 100th part of an inch in diameter, or large enough to be seen by the unassisted eye .... These at first move up and down the internode with the rapidity of animalcules, but afterwards lose this power of locomotion and become stationary. They then present, under

and thirty-seven specimens of the common Rotifer—two-thirds of which had been produced by reproduction.'

[1] See p. 384.

the microscope, the appearance of resting spores; that is to say, they consist of a dark green, globular, grumous mass, invested with a transparent spherical cell. This green mass, in all that I have examined, has been in an active state of rotation, first one way and then the other, by means of short cilia which covered its surface like those on the spores of *Vaucheria Ungeri*. . . . Two days after I had collected a number of these globular bodies and placed them in a watch-glass for observation, partly in and partly out of their respective internodes, the green mass in many had become divided up into four or more sacs, which were ciliated like the parent one, and enclosed in a second transparent spherical cell. These also rotated individually and *en masse*, while the division appeared to have enabled them to throw off the greater portion of the dark green pellets, now become black, and lying loosely in a more or less flocculent state, like effete matter, in the inner cell. . . . The third day the spherical cells had burst, and the ciliated sacs, which averaged $\frac{1}{130}''$ in diameter, were set free in the water. . . . They now presented different appearances according to their contents, shape, and motions. All were filled with a colourless, granular mucus, charged with small vesicles, and each presented also a large "contracting vesicle." In some there was left only a trace of the dark matter, while in others there was a considerable quantity, either in an undefined state, or in small globules. They presented both an undulatory motion of the cell-wall, and a ciliary motion of its

as a very diminished representative of its Oxytricha-progenitor. All the stages of this very interesting transformation are minutely described by M. Haime[1] in his memoir, and have also been very carefully delineated in numerous figures—and to these we must refer the reader who wishes for further particulars.

We may state, however, that Mr. H. J. Carter[2] seems to have witnessed somewhat similar changes, by which specimens of *Kerona pustulata*[3], after encystment, gave rise to *Plæsconia Charon*—a form closely allied to *Trichoda lynceus.* Mr. Carter hesitates about accepting such a conclusion because he did not actually see the specimens of *Plæsconia Charon* issue from the previously-observed cysts; and therefore with excessive caution—though for reasons which do not now carry much weight—he thinks that they may not have done so, and rather inexcusably suggests that M. Haime may also have made some mistake. Yet Mr. Carter had put numerous specimens of encysted Kerona aside in three separate watch-glasses, and in each of these receptacles, at about the same time that the embryos within some of the cysts began to show signs of activity, he noticed the presence of empty cysts and also that specimens of *Plæsconia* were to be seen swimming about in the watch-glasses. As not a single active Kerona could be detected, the specimens of

[1] 'Ann. des Sc. Nat.' 1853 (Zoologie), Pl. 6.
[2] 'Ann. of Nat. Hist.' 1859, pp. 251–255.
[3] See p. 242, note 1.

Plæsconia may fairly be supposed to have emerged from the empty cysts [1].

And again, the same independent reasons which tend to countenance the observations of M. Pineau also lend support to those of M. Haime and Mr. Carter. Not only may apparently similar vegetal vesicles be transformed now into Vorticellæ and now into Oxytrichæ, but, as we have seen (p. 468), others of them may be converted into the characteristic form of Plæsconia.

Other cases of such transformations will doubtless soon be made out by subsequent observers, and in addition, the altogether artificial distinctions now supposed to exist between many of the forms of Ciliated Infusoria are likely to disappear as our knowledge increases concerning their more habitual developmental phases. Systematic writers have long suspected that certain 'species,' usually described as belonging to distinct genera, are really only transitional states of other forms, which may pass into one another quite gradually without an intervening stage of encystment. A transformation of Vorticellæ into Oxytrichæ without previous encystment, has been described by Mr. T. C. Hildgard [2]; whilst I have also seen Paramecia derived from the pellicle gradually assume the characters of Nassulæ [3].

[1] There was all the less chance of a mixture of the two Infusoria having taken place originally, because the vessel, in which thousands of the *Keronæ* existed and from which the encysted specimens were taken, did not appear to contain a single specimen of *Plæsconia Charon*.

[2] 'Monthly Microsc. Journ.,' Nov. 1871, p. 232.

[3] See p. 250.

Again, simple forms like Cyclidium, Enchelys, and others, which may have been derived from Monads or Amœbæ, are also, in all probability, prone to assume very different characters when they exist under diverse conditions, so long as these conditions are not unfavourable to their growth [1].

All the evidence which has now been brought forward concerning the Ciliated Infusoria entitles us to believe that they may be derived either from lower forms such as Monads, Amœbæ, and Actinophrys; or that they may arise from the direct transformation of masses of animal or vegetal tissue existing in a separate condition, or else from parts of larger organisms which have recently individualized themselves. Whilst these transformations of larger animal or vegetal masses into Ciliated Infusoria may take place immediately, sometimes they occur only after previously-lower metamorphoses into Amœbæ or Actinophrys. The different forms ultimately assumed would seem to depend in the main upon the intrinsic molecular properties of the matter of which they are composed; whilst the extreme variability of these forms tends to imply that the molecular composition of such matter is modifiable to an extraordinary extent.

[1] Thus Prof. Schaaffhausen speaks of Amœbæ developing into specimens of Cyclidium and Chilodon, and of the conversion of the latter forms into Paramecia; though, as he says, the metamorphoses 'ne se font pas toujours de la même manière.' ('Cosmos,' t. xxii. p. 635.)

Remarks upon some of the Developmental Tendencies of Ciliated Infusoria; and upon the different Modes of Origin of Rotifers, Tardigrades, and Nematoids.

But just as the Ciliated Infusoria are often only higher developmental states of portions of living matter which have previously existed in a simpler condition, so they themselves very frequently become transformed into more complex living types. Dr. Gros pointed out these highly important facts more than twenty years ago, when, after speaking of Actinophrys, he said:—'Les utriculeux ciliés, Vorticelles, Plæsconiens, Oxytriqués, Kéronés, Dileptus, Coccudina, Nassula, etc., reconnaissent aussi une origine vésiculaire végéto-animale. En général, ils tendent aussi à l'ascendance et beaucoup (pourquoi pas tous?) se convertissent en Rotatoires minuscules, qui peuvent continuer l'échelle ascendante ou rester dans le cercle des Ciliés. Bon nombre d'Utriculeux, selon leur dérivance, s'en tiennent à tourner dans leur monde protéen et tortueux. Les agents extérieurs, la forme et la grandeur des vases sont très-puissants sur leur évolutions.'

The various forms of Ciliated Infusoria, in fact, whose mode of origin may be so different on different occasions, after living for a time a life of great activity, during which they may give birth to other similar creatures by processes of fission and external or internal gemmation, at last encyst themselves in a manner which has been so often described. Within this cyst

the contracted mass of living protoplasm undergoes processes of molecular rearrangement, varying in nature at different times, but generally resulting in a transformation into other forms[1]. Thus it may undergo segmentation into Monads or into Pythium corpuscles; it may emerge as a whole in the lower form of an Amœba or Actinophrys; or it may make its appearance as a totally different Ciliated Infusorium: whilst we have now to admit that on other occasions, as Dr. Gros long ago pointed out, the encysted mass may undergo a totally different set of molecular changes—changes of a higher kind—so that ultimately it becomes converted into an embryo of one or other of the small Rotifers. Such a transformation, according to Dr. Gros, is liable to occur occasionally amongst all sorts of Ciliated Infusoria. He says[2]:—'Il restera prouvé aussi que les Kéroniens, les Dileptiens, les Oxytriqués, les Coccudinés, etc., peuvent coconner (Pl. O, figs. 13, 4, 5; Pl. P, figs. 1–10), et donner naissance à des Rotatoires.' Whilst elsewhere he says[3]:—'Les Utriculeux dérivés des Euglènes passeront souvent les uns dans les autres, et aboutirent quelquefois à des Rotatoires stériles pour leur espèce.'

It has, moreover, long been known that the appearance of Rotifers in infusions is frequently preceded by the presence of successive forms of Ciliated In-

[1] See p. 467, note 1.
[2] Loc. cit., p. 456.
[3] Loc. cit., p. 299; see also p. 310.

fusoria. Thus, speaking of Rotifers, Pritchard says[1]:— 'They are only to be found when the first stage of decomposition has passed away, and they usually disappear again when the water becomes putrid and offensive. After the Monadinia, Paramecia, and other smaller Infusoria have run their course and in large measure disappeared, the Rotatoria occupy their places.'

I have myself been able to follow all the stages of one of the transformations, referred to by Dr. Gros, since I have seen many specimens of encysted Vorticellæ become converted into embryo Rotifers. The stages by which this metamorphosis was effected seemed to be very simple, though their real nature and the cause of the several changes was wholly inexplicable.

The phenomena were first observed under the following circumstances. A small portion of Vaucheria having been exposed to sunlight, in a beaker, for several days during the month of June, was afterwards kept in the shade and at a slightly lower temperature (about 75°F) for two days. When some specimens of it were examined microscopically, a large number of fine Vorticellæ of the striated variety were seen attached to the filaments. Other specimens of encysted Vorticellæ also existed by the side of the filaments. Some of the latter were spherical and presented the usual finely-granular appearance; whilst others, similar in size, had assumed

[1] Infusoria, 4th ed., p. 652.

an ovoidal form (Fig. 92, *a*, *b*). The largest of these measured about $\frac{1}{550}''$ in long diameter, and in them no trace of a nucleus was now to be seen, whilst the substance generally had become rather more coarsely-granular (*c*). In other specimens lying side by side with those already mentioned, the granular mass within the now-thinner envelope began, after some obscure molecular changes, though without previous segmentation, to shape itself into an embryo. This soon displayed characteristic traces of a horny pharynx and two pinkish-red pigment spots (as in *f*), whilst slow movements of the mass also became visible. When the thin cyst was ultimately ruptured and the embryo appeared, it was seen to be a Rotifer, possessing the characteristics assigned by Ehrenberg to *Diglena catellina* (*g*). Many specimens of this form of Rotifer were seen swimming about amongst the filaments, though they were all small, and when in the contracted state they scarcely exceeded in point of size the encysted Vorticellæ from which they had been derived. None of the individuals were observed to contain distinct ova, and in this respect they differed notably from specimens of the same kind of Rotifer, seen on another occasion—which were not only themselves much larger, but had also been derived from the transformation of a much larger matrix[1].

A similar transformation of striated Vorticellæ into

[1] See p. 510. In this case, too, many of the 'eggs' produced were slightly larger than the Vorticella-cysts and the embryos into which they became converted.

small Diglenæ has been observed on two or three other occasions, when the Vorticellæ had been produced from the transformation of Euglenæ, and still remained in connection with an old Euglena pellicle.

Mr. Metcalfe Johnson has moreover recently affirmed not only that Paramecia are prone to be converted into Vorticellæ, but that he has frequently seen specimens of each of these forms of Ciliata become developed into Philodinian Rotifers, without a preliminary stage of encystment [1].

Again, evidence has already been adduced sufficing to establish the intimate relationship and interchangeability existing between Actinophrys, Peranemata, and Amœbæ, as well as between each of these forms and the various kinds of Ciliated Infusoria. Not only are they freely convertible one into the other, but, now one now another of these forms may be assumed by contiguous portions of apparently similar matter—whether we have to do with Euglenæ or with individualizing portions of a dead Rotifer. If Ciliated Infusoria after encystment are therefore capable of being converted into embryo Rotifers, it is only to be expected that a similar transformation might be possible in the case of the Amœba and of the Actinophrys. And, according to

[1] See 'Monthly Microsc. Journal,' May 1871, pp. 224 and 225. The same writer refers to this subject again in the No. for Oct. 1871, at p. 187; and in the same communication he also gives reason for his belief that the divisions of the family Philodiniæ are altogether artificial, and founded upon unimportant distinctions.

Dr. Gròs, such is actually the case. Although as a general rule the Actinophrys tends to become converted into one or other of the Ciliated Infusoria, and exists in this intermediate state for a time before assuming the form of a Rotifer[1], still this is not always so. Occasionally a large Actinophrys may be transformed at once into a Rotifer or some other higher form. Their ultimate fate seems to depend principally upon their origin, size, and general vigour. The largest specimens may give off buds which become converted into Ciliated Infusoria[2], although the parent mass subsequently develops into one or other of the more complex Rotifers. Dr. Gros says:—'Quand la métamorphose ascendante approche, ils retirent successivement tous leur cils; diminutifs étonnants de suçoirs polypiens (Pl. L, fig. 9) et se transforment directement en Rotatoires.' And elsewhere[3], the same observer also speaks of the direct conversion of large Euglenæ into Amœbæ and Actinophrys, and of the ultimate transformation of some of them into Rotifers.

There are, however, other facts not yet stated which suffice to give a double interest to the transformations of Actinophrys and of Ciliated Infusoria into Rotifers.

[1] Speaking of the large specimens of Actinophrys, Dr. Gros says (loc. cit., p 436):—'Un trait qui est général, c'est qu'ils tendent tous vers les utriculeux ciliés, et ces dernières pousseront aux Rotatoires ovigères ou pangéniques selon la dérivance.' (See also p. 451.)

[2] Loc. cit., p. 436, Pl. J, fig. 16.

[3] Loc. cit., pp. 331 and 335.

We have found that the former organisms may either be produced by a kind of ascending development taking place in forms which were at first very small, or that they may come into being at once by the direct transformation of vegetal matrices equal to themselves in bulk. It would, therefore, be altogether harmonious with previously established facts if we were to find that similar laws applied to the origin of Rotifers—that is to say, if we found that they might not only arise by means of an ascending development through Actinophrys and Ciliated Infusoria, but that they might be directly derived from masses of transforming vegetal matter of the most various origin.

So long ago as 1845 Dr. Gros observed the transformation of vesicles of the third order of *Volvox globator* into a Rotifer belonging to the Philodinian type [1]; and he also called attention to the all-important

[1] See 'Bullet. de la Soc. Imp. de Naturalistes de Moscou,' 1845, p. 383 (with figures). The masses which underwent transformation seemed to correspond to the so-called 'winter-spores' of Volvox, as described by Mr. Busk ('Trans. of the Microscop. Soc.' 1853, p. 38). And according to Pritchard (p. 445) another Rotifer (*Notommata parasitus*) is occasionally found within the hollow spheres of *Volvox globator*. But it is almost impossible that a Rotifer produced from a large 'egg' could be found in such a situation, unless the 'egg' had been produced by heterogenesis. Dr. Braxton Hicks has, moreover, actually seen (*Appendix* D, p. lxxxvii.) some of the elements of the Volvox converted into large Amœbæ. So that these, as well as the so-called 'winter-spores,' might subsequently be converted into heterogenetic 'eggs.' Again, a Rotifer has been found by Rœper within the cells of the aerial leaves of *Sphagnum obtusifolium*, whilst Morren has also found specimens of *Rotifer vulgaris*

fact that in such a transformation the germ is almost equal in bulk to the fully-developed animal to which it gives birth. Again, at a later period (1859), in the memoir to which I have so often referred, Dr. Gros frequently alludes to similar direct transformations of large Euglenæ into Rotifers of the most varied nature and size[1]. He says that the Euglenæ which undergo transformation gradually decolourize, and at the same time the matter entering into their composition becomes more and more animalized[2]. They either retain the spherical form or else become slightly oval, whilst the internal substance soon begins to exhibit contractions with traces of organization, and at last an embryo Rotifer is distinctly visible[3]. Elsewhere Dr. Gros says[4]:—'On voit donc l'Euglène prendre la forme ovalaire (Pl. H, Fig. 1), passer par la décoloration

within closed filaments of Vaucheria—a fact which had been partially indicated by Unger in 1828. Speaking of the filament in which it was contained, Morren says:—'An attentive and lengthened observation convinced me that in this there was no solution of continuity, and that the arrival of the Rotifers within the Vaucheria was not at all to be explained in this way. How are these parasitic animalcules generated within them? This is what further research has some day to show.' (See Pritchard's 'Infusoria,' 4th ed. p. 468.) Pritchard (p. 464) also says that a form of *Notommata* has been found by Perty within the filaments of a Vaucheria, and that another form of Rotifer (*Albertia vermicularis*) is commonly found within the intestines of earth-worms and slugs.

[1] See loc. cit., pp. 332, 449; Pl. D, figs. 37-40, and Pl. C, figs. 1-12.

[2] Loc. cit., p. 307.

[3] See also loc. cit., pp. 310, 314, 318.

[4] Loc. cit., p. 324.

ordinaire, et elaborer ses vésicules internes (Fig. 4), qui deviennent huileuses, qui se scindent (Fig. 5) et se revésiculisent, jusqu'à ce que le degré d'animalisation convénable soit atteint, et propre à donner naissance aux Rotatoires les plus variés, selon la taille et la forme

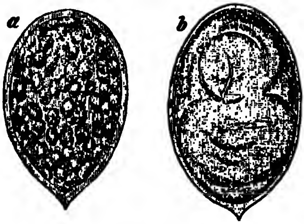


FIG. 91.

Conversion of Encysted Euglena into a Rotifer. (Gros.) (× 500.)

a. Encysted Euglena of an oval form, and pointed at one extremity.
b. Similar body after its contents have become animalized and converted into an embryo Rotifer. The whole mass is now larger and the investing envelope thinner, though it still remains pointed at its posterior extremity.

de l'œuf.' So multitudinous are the forms of Rotifera which he has seen emerge from these transforming Euglenæ that Dr. Gros says:—'L'on peut se demander s'il existe une forme de Rotatoire qui ne puisse dériver des Euglènes directement ou par d'autres transformations[1].' Thus, in addition to the numerous kinds of

[1] Such notions are certainly in harmony with facts long known concerning the habitats of Rotifers. Speaking on this subject Pritchard says ('Infusoria,' 4th ed. p. 653):—'Usually they must be sought for in a systematic way, without any external indications whether a pool will prove productive or barren. We have, however, rarely been disappointed

small Rotifers, even the largest forms—such as Brachion and Hydatina—have frequently been seen to arise from the transformation of large specimens of Euglenæ, both rose-coloured and green. Some of these when contracted formed spheres as much as $\frac{1}{210}''$ in diameter, and the transformation took place either with or without the previous formation of an enveloping cyst—though generally under the former conditions[1]. The real nature of the cyst was, moreover, in certain cases quite obvious, even at a late stage, owing to its containing an unmetamorphosed portion of the original Euglena substance, in contact with the embryo Rotifer.

The next reference to the heterogenetic origin of Rotifers occurs in the writings of M. Nicolet, to which we have already referred[2]. He says that such organisms are often met with amongst the multitudinous forms of Infusoria which he has seen budded off from the closed internodes of Chara; and he figures the kind

on examining the green and foul-looking drainage from the manure-heap in the farm-yard. Amidst its swarms of Euglenæ we have usually found a rich supply of Rotatoria.'

[1] Speaking of such transformations which were seen in Germany during the year 1852, and in the month of August, Dr. Gros says:—'En général, à cette époque d'observation, se produisaient surtout les grandes espèces de Systolides, ceux dérives des Euglènes entières transformées, depuis les Hydatines jusqu'aux plus grand Brachions. Dans quelques-uns de ces individus nées sans parents, on percevait déjà les œufs de leur future lignée, et *ces œufs, soit dans le corps maternel, soit pondus, ne pouvaient être confundus avec ceux résultant des transformations Eugléniennes, qui ont d'ailleurs été suivées non sur quelques individus, mais on peut dire sur des millions.*' (See also loc. cit, pp. 321, 324, 325, and 333; Pl. C', figs. 8 and 13.)

[2] See p. 478.

of bud from which they are especially prone to be developed (see Fig. 87, *d*). No further details of the process, however, are given.

I have seen none of the very large Euglenæ described by Dr. Gros, and consequently have not been able to observe the direct mode of origin of Rotifers from this particular kind of matrix. Such transformations are, however, by no means confined to Euglenæ. Similar changes were seen by Dr. Gros taking place in large vegetal vesicles thrown off from Mosses [1], and I have also been able to trace two distinct modes of origin of Rotifers from different algoid matrices.

In a vessel containing an abundance of Chlorococcus at the sides and on the surface of the fluid (to which allusion has already been made), there were, in addition to the beautiful vesicles $\frac{1}{1000}''$ in diameter, which became transformed into Oxytricha and Plæsconia [2], a number of others varying between $\frac{1}{800}''$ and $\frac{1}{500}''$ in diameter—these being evidently larger specimens of the same kind. Many of them were composed of a dense aggregation of the brightest green corpuscles packed within a thick, colourless, cyst-like envelope (*e*). Amongst these green specimens, however, others might be seen undergoing various stages of decolourization. The chlorophyll at first seemed to become preternaturally green, though it afterwards gradually assumed a bright orange tint—whilst the contained corpuscles soon gave place to granules, and the whole cyst underwent a slight increase

[1] See loc. cit., p. 449; Pl. I, figs. 4–7. [2] See p. 467.

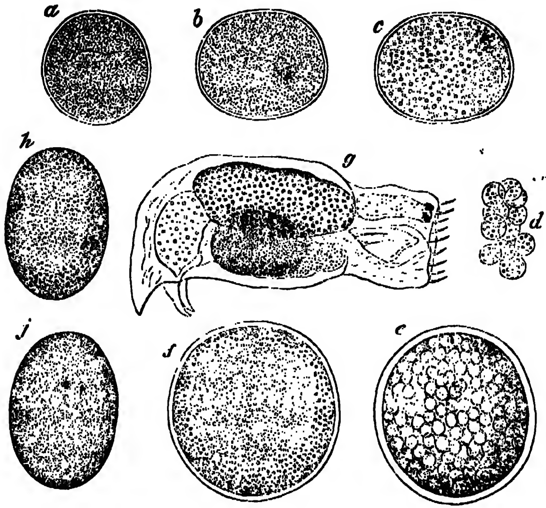


FIG. 92.

Origin of similar Rotifers, from Vorticellæ, and from the direct Transformation of Algoid Corpuscles. (x 600.)

a. Encysted Vorticella, which subsequently may become ovoid and more coarsely granular, as in *b*, *c*, preparatory to the conversion of the encysted mass into an embryo Diglena, closely resembling *j*.

d. Small Chlorococcus corpuscles.

e. A very large, bright green Chlorococcus vesicle, $\frac{1}{550}''$ in diameter, which subsequently becomes decolourized into a mass of orange-brown, granular protoplasm (*f*), whilst this subsequently becomes converted into an embryo *Diglena catellina*, a little smaller than the adult form represented at *g*. This adult form contains a developing 'gemma,' which when laid resembles *b* both in size and in appearance.

j. Represents the appearance presented by one of these gemmæ (having a very thin investing membrane) after it has been directly converted into an embryo Diglena, in which pink eye-specks and a rudimentary pharynx are already visible.

in size (*f*). Other cysts were seen, representing later stages. Each of these was densely packed with rather coarse yellowish-brown granules; and, after a time, the whole mass began to shape itself into an animal organism, irregularly folded, but presenting all the appearance of being an embryo Rotifer furnished with two red pigment spots. The body had by this time still further increased in size, and the cyst-wall had become proportionately thinner, so that, after the usual struggling movements of the embryo, the cyst was at last ruptured, and a Rotifer appeared—notably larger, though otherwise similar to the Diglena[1] which, as we have already seen, may be produced from an encysted Vorticella. The vessel contained multitudes of these Rotifers; only, unlike the small forms produced from Vorticellæ, many of them were seen to contain a single large 'egg' or gemma[2]. These, however, had the usual delicate wall and more

[1] These organisms were generally about $\frac{1}{250}''$ long by $\frac{1}{600}''$ broad.

[2] I entirely agree with Prof. Cohn concerning the nature of these bodies. According to Pritchard (loc. cit., p. 656), 'Dr. Cohn contends that the bodies ordinarily regarded as eggs are merely gemmæ thrown off from the organ believed to be an ovary, without any fertilization by a male animal.' Prof. Huxley, on the other hand, whilst he regards these bodies as eggs ('Trans. of Microsc. Soc.' 1853, p. 14), considers some of the so-called 'winter-eggs' to be real gemmæ produced by the individualization of a portion of the ovary. In the face of the observations of Dr. Gros, however, it is important to note that the 'winter-eggs' described by Prof. Huxley were never seen by him to give birth to a Rotifer. On the other hand, we shall subsequently find that many specimens of the so-called 'winter-eggs' (found in the free state) have been produced heterogenetically, and not by Rotifers, although they have been seen to give birth to Rotifers.

surface. Sometimes the cilia were motionless, and lay like a halo of short radii round its circumference, though

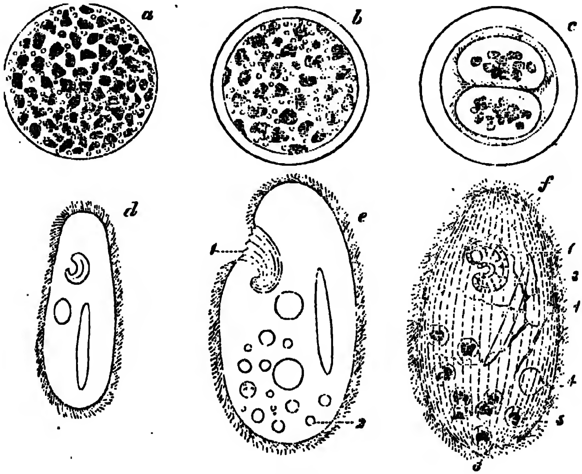


FIG. 38.

Mode of Origin (?) and Development of Otostoma. (Carter.)
(× 200.)

a. Nitella-spheres. Cilia not represented.
b. Smaller body of same kind enclosed within a cyst, which at length becomes transformed into two ciliated embryos (*c*).
d. Outline appearance of embryo Otostoma soon after its emergence from the cyst.
e. More developed form of same—lateral aspect.
f. Front view of adult Otostoma—showing ear-shaped buccal orifice, and contracting vesicle with radiating canals.

the sac was otherwise gradually changing its shape; while at others there was no appearance of cilia at all. On the other hand, sometimes the sac was rotating

rapidly under a globular form, with its wall undulating and cilia playing over it with corresponding activity. . . . Occasionally, a sac might be seen under an elongated, oblong form, with a slow undulatory change of shape at one end, and a languid movement of the cilia on its surface generally; again it might be seen with mucus-radii spread out in the same way as those of *Actinophrys sol.*' Subsequently Mr. Carter tells us that the almost endless modifications assumed by the sacs were dependent upon their having been forced from their cysts before they were developed. He afterwards examined specimens very carefully which had been liberated from the parent cysts in the natural way, and then they were found to be veritable ciliated Infusoria closely allied to Paramecium, to which Mr. Carter gave the generic name Otostoma, on account of the resemblance between the shape of the oral orifice and that of the human ear. The segmentation of the protoplasm within the parent sac is always into two, four, or eight embryos; and these, when first liberated from the cyst, present only one contracting vesicle. Subsequently the mouth becomes more defined, and a second contracting vesicle is produced [1].

[1] As in other instances, since the publication of these observations Mr. Carter has endeavoured to put a different interpretation upon the facts (see 'Ann. of Nat. Hist.' vol. viii. pp. 285 and 288). No feasible explanations, however, are offered as to the means by which such large Infusoria could have made their way into the closed chambers of the Nitella, nor is it easy to believe that, after having got in, they would all exhibit a gluttony so extreme as to account for the appearance of the ciliated sacs as seen by Mr. Carter. Such a state of extreme engorgement

If, however, we find that such varied organisms as those mentioned by Carter and Nicolet, and those which I have previously described [1], may be produced by transformations of different portions of the same Nitella or Chara substance, it may be less an occasion for surprise when we ascertain that in other cases many similar kinds of Ciliated Infusoria may proceed from what appears to be a totally different sort of matrix—namely, from a mass of protoplasm which separates directly from some animal or from some animal-product. Ciliated Infusoria have, indeed, been frequently seen by Dr. Gros to develop from individualized portions of dying Rotifers or Tardigrades, as well as from portions of the embryos of the common earth-worm [2] and of the eggs of Rotifers, although the units of matter which individualize themselves may, in the first instance, take on the form either of an Amœba, an Actinophrys, a Peranema, or an Arcella.

After the larger Rotifers have laid their eggs and run through their ordinary term of existence, they do

with food I have never seen—and its existence to any notable extent amongst these forms of life is somewhat rare. Mr. Carter very kindly placed some additional facts concerning the structure of these organisms at my disposal, which I regret that I am now unable to mention in detail. He will, perhaps, enter upon them in some future communication.

[1] See pp. 401–406.

[2] We have already (p. 337, note 1) alluded to some forms of Ciliated Infusoria which have been found within the bodies of higher organisms, and which very probably have been derived from the transformation of some elemental portion of the higher organism. And again we may ask, What is the mode of origin of the Leucophrys which habitually exists within the perivisceral cavity of the earth-worm?

not wholly die. According to Dr. Gros, they may resolve themselves, by what he very aptly termed a process of 'pangenesis,' into different lower forms of life[1]. Previously to undergoing these transformations they contract into a ball-like form, whilst their alimentary canal, glands, and all other internal parts undergo a new molecular elaboration and rearrangement, followed by a process of segmentation, whereby the altered and rejuvenized substance is converted into independent masses of living matter capable of passing on to new and varied forms of life. As a result of this process, according to Dr. Gros[2], comparatively large and more or less spheroidal masses of protoplasm (Amœbæ) may be seen making their way through the integument of the dead Rotifer; though at other times the masses assume different forms whilst still within the old integument. But in either case some of the individualized and amœboid bodies assume the form of large specimens of Actinophrys, whilst others may become converted into specimens of Peranema. The Actinophrys is generally represented most abundantly, and sometimes such forms exist without any admixture with Peranemata. On other occasions, though much more rarely, all the individualizing masses may take on the form of Peranemata.

The Peranemata are colourless, flagellated organ-

[1] See pp. 382 and 393, for observations on the same subject by M. Nicolet and Mr. Carter.

[2] Loc. cit., pp. 430–433; Pl. M, figs. 1, 2.

isms[1] (Fig. 89, *g*) which increase in size, and subsequently make their way through the integument of the Rotifer in which they have been formed. They take all varieties of food lying in their way, after the manner of an Amœba, and thus increase considerably in size. They then assume a spheroidal shape and protrude pseudopodia, so as to become converted into Actinophrys, which, after still further increasing in size, retract their rays, undergo an internal elaboration, and then develop cilia on different parts of their surface. They thus become converted into specimens of the higher Ciliated Infusoria belonging to the families Keronæ or Oxytrichæ. And, similarly, specimens of Actinophrys which have originated more directly from the Rotifer may also, according to Dr. Gros, pass through such transformations. Other specimens, however, attain a very large size, and are often seen to detach portions of their substance which have the power of developing into one or other of the forms of Ciliated Infusoria, although the larger parent-masses may undergo different transformations[2].

But other Rotifers, instead of giving birth to specimens of Actinophrys and Peranema, or to either of these forms alone, may become resolved into a number

[1] And, according to Dr. Gros (loc. cit., p. 435), they may arise in many different modes. See pp. 459 and 549.

[2] Dr. Gros says:—'S'il n'est pas toujours possible de dire, à première vue, ce que doit devenir tel Actinophrys, puisque la transformation finale tient à la taille, à la dérivance, à l'abondance de nourriture, etc., un trait qui est général, c'est qu'ils tendent tous vers les utriculeux ciliés.' (p. 436.)

of the encysted Amœbæ known as Arcellinæ [1]. In this case, from the transforming substance of the dead Rotifer portions of matter (*a*, *b*) bud out, which, when they have attained a certain size (*c*), very frequently divide into two parts, each of which becomes more and more condensed externally, so as to produce an Arcella with the characteristic cyst-like though perforated envelope (*d*, *e*). The forms of Arcellæ produced in this

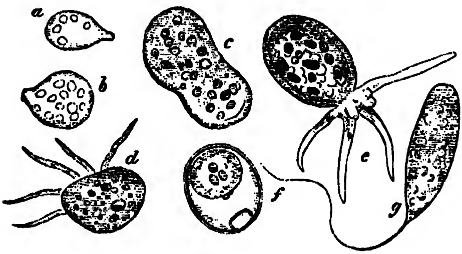


Fig. 89.

Arcellæ and Peranema derived from Pangenesis of Rotifers. (Gros.)

a, *b*. Bud-like outgrowths from the substance of a dying Rotifer, which increase in size and at last form a mass (*c*), which divides into two equal parts, each of which speedily takes on the form of an Arcella (*d*).

e. More completely developed form of Arcella.

f. An Arcella whose animal substance has contracted within its shell preparatory to further transformations.

g. A Peranema produced from an individualized portion of the substance of a dying Rotifer.

manner are of the most varied nature, though they all protrude portions of their internal substance through

[1] They form a group of organisms which intervene between ordinary Amœbæ and those living in very complex chambered cells, which are known by the name of Foraminifera.

apertures in their shell-like envelope. After they have attained a certain size, and after their internal substance has undergone a certain molecular elaboration (of the nature of which we are wholly ignorant), any one of them may quit its cyst and contract into a spheroidal or ovoidal mass, which soon protrudes cilia, and develops into one or other of the numerous varieties of Plæsconia or Oxytricha. Whilst, if of smaller size when it quits its cyst, the mass may live for a time as an Amœba, during which it grows and gradually acquires a sufficient bulk and molecular elaboration to enable it to become transformed into one of the above-mentioned Ciliated Infusoria.

Arcellinæ of a similar kind were also seen by Dr. Gros[1] to be produced abundantly from the substance of certain young embryo earth-worms, which had been in his possession (in the egg-state) for more than eighteen months. These Arcellinæ, like those produced from the substance of Rotifers, also subsequently gave rise to Ciliated Infusoria, either directly or after a previous amœboid phase of existence[2]. Whilst, on the other hand, the observations of M. Vogt and M. Nordmann long ago revealed the fact that bodies resembling Ciliated Infusoria were occasionally budded off from the early embryonic mass of certain of the Gasteropoda. In referring to these observations Dr. Carpenter

[1] Loc. cit., p. 433.

[2] Prof. Agassiz also declares that he has seen Ciliated Infusoria derived from eggs of Planariæ (see *Appendix* D, p. 107).

says[1]:—'It is not unfrequently seen that some of the cells of the vitelline mass detach themselves from the principal cluster, become clothed with long cilia, and continue to move about actively within the egg until the escape of the embryo. It is even affirmed by Nordmann that they increase by partial subdivision, and that thus from a single detached cell may be produced a cluster having a very definite form, and furnished with long cilia, so as very strongly to resemble a parasitic animal.'

The independent observations of Dr. Gros have, moreover, established the fact of the occurrence of analogous phenomena amongst the Rotifera. He says he has seen a large egg, which had remained within the envelopes of the parent for about three months after its death, at last begin to produce buds on its external surface[2]. These buds continued to increase in size, and after separating became converted into Ciliated Infusoria. And on another occasion, having in his possession a number of heterogenetic Rotifer germs[3] which had been corked up in a bottle during a long journey (and thus exposed to very unfavourable conditions), Dr. Gros found by subsequent examinations that these matrices did not go on to the development of Rotifers, as hundreds of them had previously done. He says[4]:—'Le vitellus du futur Rotatoire élabora bien encore ses

[1] 'Principles of Comp. Physiol.,' 4th ed., p. 580.

[2] Loc. cit., p. 451, Pl. O, figs. 6, 7, 8.

[3] That is to say, a multitude of large Euglenæ, very many of which had become animalized and converted into embryo masses, such as usually develop, and formerly had developed, into Rotifers.

[4] Loc. cit., p. 329.

vésicules; l'animalisation progressa, mais non plus en faveur d'un organisme individuel supérieur; et au lieu de voir le contenu de cette chrysalide donner un Rotatoire comme leurs congénères antérieures, on vit le vitellus se résoudre à l'intérieur du cocon en des organismes inférieurs, ou pousser à l'extérieur du cocon des utricules hilés, qui devenaient la source d'une nouvelle génération d'Infusoires utriculeux[1].'

But quite recently I was fortunate enough to observe somewhat similar phenomena. The substance of some of the large thin-walled 'eggs' of *Hydatina senta* was seen to have undergone segmentation into about sixteen spheres, each $\frac{1}{1000}$ in diameter. The external layers of these soon became condensed into cyst-walls, whilst the internal substance of each of them, after undergoing a series of molecular changes, resolved itself into an embryo Oxytricha, some of which might be seen revolving within their cysts. Some of this batch of Rotifer 'eggs' were seen to be filled with such spheroidal masses, whilst others were observed in which a few of the embryos had escaped from their cysts, and were swimming about as well-marked specimens of Oxytricha within the thin investing membrane of the Rotifer egg[2]. And on another occasion, when some

[1] Similar utricles developing into Vorticellæ may, moreover, according to Dr. Gros, be budded out even from Euglenæ which have undergone no decolourization, and which, therefore, have no actual relation to Rotifers. (See loc. cit., p. 475, Pl. C', fig. 10.)

[2] These organisms were almost precisely similar to those which proceeded from the Chlorococcus vesicles (see p. 467).

Hydatina 'eggs' had been kept for a time within a developmental chamber (in which they had been exposed to unnatural conditions), their substance underwent segmentation into a multitude of Monads. Dr. Gros[1] has, moreover, observed that Rotifers and Rotifer-germs occasionally become putrid and resolve themselves into a dense swarm composed of thousands of Bacteria.

The facts just mentioned are thoroughly in accordance with previous observations, from which we have learned that an organic matrix capable of giving birth to a higher form may, when subjected to the influence of more and more unfavourable circumstances, give birth to lower and lower forms[2]. But although analogous phenomena have been already recorded, there is room for surprise when we find that the egg of one of the largest and most complex of the Rotifers, instead of undergoing its own proper phases of development, may, under one set of comparatively unfavourable conditions, yield a dozen or more Ciliated Infusoria, whilst under still less propitious influences it may produce hundreds of active Monads, or even resolve itself into a swarming brood composed of thousands of the simplest living units.

[1] Loc. cit., pp. 440, 472.

[2] An 'embryonal sphere' derived from Nitella may for instance be transformed into a Ciliated Infusorium, though under less favourable conditions it either segments into Monads or becomes resolved into a swarm of Bacteria—to say nothing of other possible modes of transformation. (See pp. 401–406.)

CHAPTER XXII.

TRANSFORMATIONS OF CILIATED INFUSORIA: MODES OF ORIGIN OF ROTIFERS, TARDIGRADES, AND NEMATOIDS.

Convertibility of Lower Forms of Life. Similar Convertibility of Ciliated Infusoria. Vorticella into Oxytricha. Oxytricha into Trichoda. Mr. Carter's Observations. Recent Confirmation of these neglected views. Other Developmental Metamorphoses. General Conclusions concerning Ciliated Infusoria. Their ultimate Transformations.

Origin of Rotifers. Dr. Gros' Observations. Confirmed by Transformation of Vorticella into Diglena. Conversion of Actinophrys into embryo of Rotifer. Fate of other large specimens of Actinophrys. Direct Origin of Rotifers from Vegetal Vesicles. Conversion of 'winter-spore' of Volvox into Philodinian Rotifer. Similar Transformations of Euglenæ or of Moss-sporangia into other Rotifers. Author's Observations on so-called 'winter-eggs' of Hydatina. Their Mode of Formation from aggregations of Chlorococcus Vesicles and of Euglenæ. Instances of Synthetic Heterogenesis. Reproduction amongst Rotifers. Transformation of Actinophrys into Planariole, and of Planariole into Tardigrade. Similar Origin of Nematoids. Transformations of Small Rotifers into Nematoids. Direct Origin of Tardigrades and Nematoids from Euglenæ. Indubitable Nature of these Changes. Author's Observations on Origin of Nematoids. Conversion of 'resting-spore' of Vaucheria into an embryo Diplogaster. Inexplicable Facts. New Views harmonious with previous Observations. Universal Distribution of Rotifers, Tardigrades, and Nematoids. Failure of the Panspermic Hypothesis. Heterogenetic Origin of other Higher Forms. Unsolved Problems.

THE facts hitherto recorded concerning the modes of origin of Ciliated Infusoria are of such a nature as to lead us to expect that these varied forms

might be to a certain extent mutually convertible. We know that different portions of the same mineral substance in a state of solution may, if their ultimate molecular arrangement becomes affected by exposure to different conditions, aggregate into quite different crystalline forms; and that, having aggregated under one of these forms, they are often capable, under the influence of further changes, of lapsing into another and entirely different crystalline state[1]. And similarly, it has also been ascertained that Monads, Actinophrys, Peranemata, Amœbæ, and Fungus-germs frequently proceed from contiguous portions of the same matter, whilst these several forms are, moreover, to a very notable extent interchangeable with one another[2]. Again, we have ascertained that many of the Algæ, Desmids, Pediastreæ, and Diatoms (and probably even many Lichens and Mosses) may proceed from different portions of the same kind of matter, and that such modes of growth are also to a certain extent mutually interchangeable[3]. So that, after having discovered that totally different forms of Ciliated Infusoria may arise from contiguous and similar algoid vesicles or other matrices, we have a right to expect that evidence would also be forthcoming as to the convertibility of some of these forms.

[1] See pp. 57 and 82. We have also endeavoured to show that a somewhat similar relationship exists between the primordial living things known as Bacteria, Torulæ, Vibriones, &c. (pp. 136–143).

[2] In Chaps. xvii. and xx.

[3] See pp. 412-423, and 441-455.

And, as a matter of fact, phenomena of this kind have been long ago and quite independently recorded by different investigators. Hitherto such observations have been almost wholly discredited—not because there was any reason to suppose that due care had not been exercised by those who made them, but simply because the facts recorded were not in harmony with the theoretical views held by the majority of biologists. This rejection of facts which do not accord with generally received theories is unfortunately only too common.

Two sets of observations which were made many years ago, and quite independently of one another, are to a certain extent complementary. The metamorphosis of Vorticella into Oxytricha was described by M. Pineau in 1848; whilst the metamorphosis of Oxytricha into Trichoda was afterwards watched by M. Jules Haime in 1855.

M. Pineau's observations[1] were made upon specimens of Vorticellæ which had been developed in great numbers in an infusion of *Aconitum napellus*. Some of them were seen to undergo longitudinal fission and produce buds in the usual manner, though others after a time passed into the encysted condition. The body contracted, assumed a spherical form, and produced a secretion which soon solidified into a tolerably thick cyst-wall, whilst the pedicle shrivelled and gradually disappeared (Fig. 90, *a*, *b*)[2]. These encysted Vorticellæ

[1] 'Ann. des Sc. Nat.' 1848 (Zool.), p. 99.

[2] On other occasions a posterior ciliary wreath is developed, and the organism separates from its pedicle before it begins to encyst itself.

slowly increased in size, whilst at the same time the cyst-wall became thinner and thinner; and the contained mass, after having assumed a mamellonated appearance, again became finely-granular (*c*, *d*). After a time the embryos began to rotate within their cysts, although at this stage no cilia were to be detected. Such changes were frequently observed, but it was some time before M. Pineau was enabled to make out any further alterations. The infusion contained, in addition to the multitudes of Vorticellæ, a crowd of much more minute organisms, amongst which were included Monads and small Amœbæ. But there were also some large specimens of Oxytrichæ, whose dimensions far exceeded that of any of these other organisms. The mode of origin of the Oxytrichæ was for a time just as obscure as the ultimate changes undergone by the contents of the Vorticella cyst, till at last M. Pineau discovered other spherical bodies (*e*) in the midst of a mass of the oviform cysts, equal in size to the largest of them, 'mais denués d'enveloppe, et munis de cils gros et rares, qui rapellaient entièrement ceux des Oxytriqués; si ce n'est que leur mouvements étaient lents, et que leur consistance paraissait plus molle.' M. Pineau adds:—'Sur d'autres globules, les cils formaient, en outre, une bande diagonale, indice de celle qui accompagne la bouche chez les Oxytriqués entièrement développés. . . . . . Enfin on en voyait qui affectaient des formes plus ou moins ovalaires, de telle sorte qu'on arrivait par des transitions insensibles à la forme de

l'Oxytriqué parfait.' After these observations M. Pineau felt compelled to believe that the rotating embryos seen

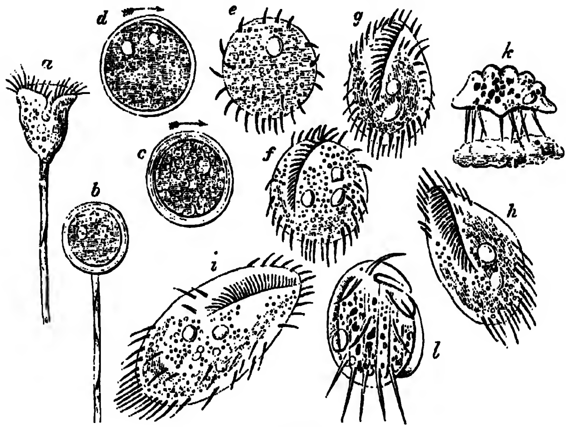


FIG. 90.

Transformations of Ciliated Infusoria. (Pineau and Haime.)

a–i. Representing stages by which Vorticellæ became converted into Oxytricha. (Pineau, x 265.)

k and *l*. Trichoda, seen laterally and from its under surface, such as was seen to result from the transformation of other specimens of Oxytricha. (J. Haime, x 750.)

within the Vorticella cysts had, by a further metamorphic process, been converted into these embryo Oxytrichæ, and Mr. T. C. Hildgard has, of late, apparently seen some of the stages of a similar transformation[1]. We now know, moreover, that a Vorticella and an Oxytricha may result from the transformations of two

[1] See 'Monthly Journal of Microsc. Sc.,' Dec. 1871, p. 281, and Silliman's 'American Journal,' August, 1871.

contiguous Euglenæ or other algoid vesicles, which, previous to their decolourization, had appeared in all respects similar to one another[1]. Nay more, whilst we are told by Mr. Metcalfe Johnson that Paramecia very frequently become transformed into Vorticellæ[2], Mr. T. C. Hildgard states that some Paramecia may also give rise to Oxytrichæ[3].

Again, it must not be supposed that all Oxytrichæ tend to become transformed into specimens of Trichoda, although the very careful observations of M. Jules Haime fully entitle us to believe that some Oxytrichæ may undergo such transformations[4]. He has shown that one of these organisms, after it has become encysted, may undergo a long series of changes, during which effete matter is frequently thrown off, and that ultimately a specimen of *Trichoda lynceus* (Fig. 90, *k*, *l*) appears

[1] It is interesting to notice that these Oxytrichæ began their existence in a comparatively embryonic form, whilst those which arise from the transformation of Chlorococcus vesicles, emerge from their cysts in a condition more closely resembling that of the adult.

[2] 'Monthly Microsc. Journ.,' May, 1871, p. 223.

[3] 'Monthly Microsc. Journ.,' Dec., 1871, p. 280.

[4] After animadverting very justly upon the strong terms of condemnation which M. Claparède made use of with respect to these observations of M. Haime—more especially when Claparède's condemnation was not based upon any observations of his own enabling him to explain the supposed sources of error into which M. Haime had fallen—Quatrefages ('Metamorphoses,' translated by Dr. Lawson, 1864, p. 188) says:—'The conscientious manner in which this young naturalist laboured is well known, and I am personally aware that the memoir in question occupied a very considerable portion of his time, and that he took the greatest precautions to isolate the objects under observation, and to avoid all possibility of mistake.'

evenly-granular appearance, and none of them were larger than $\frac{1}{500}''$ long by $\frac{1}{800}''$ broad (*b*). They differed, therefore, in the usual manner from the heterogenetic matrices from which their parents had been derived; and no mistake could have occurred, since the gemmæ were seen and measured within the body of the parent as well as after they had been laid, when the development of the embryo within them was also watched (*j*). These gemmæ gave rise to organisms about the same size as those which had proceeded from the encysted Vorticellæ[1].

Other observations made upon the contents of the same vase were, however, even still more interesting, since they have sufficed to reveal the heterogenetic origin of one of the largest and best known of the Rotifers.

[1] With reference to other direct heterogenetic modes of origin of Rotifers, I may state that I have again and again seen small Rotifers within the still-closed though dead internodes of Nitella, which I have every reason to believe were produced by the direct transformation of some of the larger embryonal spheres that had been formed within the filaments (see p. 401). The form of Rotifer most frequently seen under these conditions was one with a shield-like carapace and single style, apparently corresponding to the so-called *Monostyla cornuta*. Again, quite recently, whilst engaged in the investigation of certain transformations taking place in Vaucheria, I accidentally encountered a number of young germinating spores, nearly every one of which, though still green externally, contained a moving embryo Rotifer in its interior. The spores were of the kind represented in Fig. 6, *k*, and they were only very slightly more advanced in development. The diameter of the contained embryo was about equal to one-half of the diameter of the spore. I saw more than a dozen of these bodies, but unfortunately, owing to my occupation with another series of transformations at the time, I did not ascertain anything further concerning them.

The vessel contained, as I have said, an abundance of Chlorococcus growing in jelly-like masses and also in other modes. There was, for instance, a very large quantity of small green vesicles—from $\frac{1}{7000}''$ to $\frac{1}{5000}''$ in diameter—which did not exist in any obvious jelly-like matrix. Some of these vesicles grew out into elegant Confervæ, whilst others progressively increased in size so as to yield the much larger vesicles from which the Ciliated Infusoria and the Rotifers were derived. Multitudes of these small vesicles, however, did not increase in size at all, owing to the continuance of processes of fission, though they remained in a state of aggregation. They thus formed masses which increased in size till more or less spheroidal heaps were produced about $\frac{1}{200}''$ in diameter, partly separate and partly in apposition with one another.

A pellicle on the surface of the fluid was in the main composed of these various elements; and they also were the principal components of the thin layer by which the glass beaker was lined. But, thickly interspersed amongst some of the heaps of minute Chlorococcus-vesicles, there were a number of dark-brown ovoidal, egg-like bodies, also about $\frac{1}{200}''$ in diameter. I soon satisfied myself that these were the so-called 'winter-eggs' of the beautiful and highly complex Rotifer known by the name of *Hydatina senta*[1]. Such 'winter-eggs' were

[1] These observations were made during the month of April 1872, and the fluid in which the Chlorococcus had developed had been placed in the beaker about six weeks previously.

originally described by Ehrenberg, who recognized that the process of development went on within them at a much slower rate than it did within such large ova or gemmæ as are ordinarily produced within the adult animal. These gemmæ also possess a smooth transparent and very thin envelope, whilst in the so-called 'lasting or winter-eggs' the external surface is 'hairy' or villous, and the envelopes are double, in addition to being much thicker and more opaque.

After some careful investigation, I ultimately ascertained that every intermediate stage was to be seen, between the spherical or ovoidal heap of green Chlorococcus-corpuscles (Fig. 93, *a*) and the fully-formed winter-egg containing an embryo Hydatina, whose ciliary wreaths were in full activity. The steps of the transformation were as follows:—The mass of aggregated algoid vesicles assumed a preternaturally bright-green colour, and some of the corpuscles seemed to fuse with one another, owing to a solution of their thin envelopes—so that the masses after a time presented a more granular appearance. Decolourization then seemed to commence and proceeded rapidly throughout the portion of algoid matter which was destined to form the future 'egg'—always involving an ovoid mass about $\frac{1}{200}''$ in diameter. The external limits of this transformation were always sharply defined, and I have seen many bodies in this early stage in which the future egg was represented by an opaque ovoidal mass of rather dark-brown granular matter, to which no bounding membrane

existed, and which was fringed externally by adherent and unmetamorphosed green corpuscles (*b*). In other

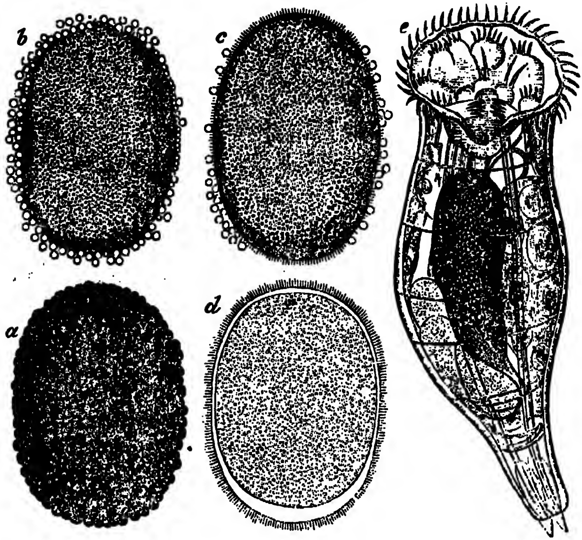


Fig. 93.

Transformation of a mass of Chlorococcus Corpuscles into the so-called 'winter-egg' of *Hydatina senta.* (x 250.)

a. Ovoid mass of bright-green Chlorococcus corpuscles, about $\frac{1}{200}''$ in long diameter.

b. Such a mass after its transformation into a brown granular body, without distinct bounding wall. (Should have been intermediate in tint between *a* and *c*.)

c. A similar body at a later stage, when a limiting envelope has made its appearance, upon which villous outgrowths had been produced.

d. Later stage, constituting the so-called 'winter-egg' of *Hydatina.*

e. *Hydatina senta* which is produced from such a body—almost adult.

specimens the first traces of a bounding membrane appeared, though such bodies were still more or less

surrounded by adherent Chlorococcus-corpuscles (*c*). And the membrane having been once formed, minute but densely-packed villous outgrowths soon appeared upon its surface. This membrane gradually thickened, whilst the substance of the egg became a little less opaque, and after a time the internal contents separated from one end, so as to leave a space between the thick outer shell and the inner membrane by which the evenly granular embryo mass was now enclosed (*d*). Later still[1] the embryo mass becomes lighter and more refractive, whilst it is seen to move within the cyst, and the play of its ciliary wreaths may also be distinguished—though with some difficulty, on account of the opaque nature of the cyst in which the embryo is contained. When the cyst is ultimately ruptured, a well-organized specimen of *Hydatina senta* makes its appearance.

Concerning the reality of all these transformations, astounding as they are, I now entertain not the slightest doubt. And, in addition to having traced all the stages by which the heap of Chlorococcus-corpuscles becomes transformed into an embryo Rotifer, I have over and over again ascertained, from an examination of different portions of the pellicle on the surface or at the sides of the vessel, that the number of the green heaps and of the brown egg-like bodies in different stages was always

[1] The duration of which is uncertain, since, whenever I have attempted to isolate specimens, so as to try to watch their individual development, I have found that the transformative changes were arrested. Similar difficulties have also been encountered in attempting to watch the development of other forms. (See also Gros, loc. cit., pp. 334-337.)

inversely proportionate to one another. In portions that were green, the algoid aggregations were numerous, and the 'winter-eggs' were scarce; whilst, as more and more decolourized portions of the previously green pellicle were selected for examination, the 'eggs' increased in number and the green Chlorococcus-heaps became scarcer and scarcer.

But even before I had thoroughly satisfied myself concerning this mode of origin of Hydatina, I had had evidence almost as convincing—which at the time I could scarcely bring myself to credit—that these splendid Rotifers are occasionally produced, in an almost similar manner, from aggregations of small Euglenæ[1] entering into the composition of a Euglena-pellicle. Brownish egg-like masses, about $\frac{1}{200}''$ in length, were apt to appear imbedded in the very midst of the pellicle. They were somewhat variable in size, and, like the bodies we have just been describing, they were more decidedly brown in colour than those which are produced within an adult Hydatina. The difference, however, was not nearly so marked between the ordinary gemmæ and the so-called 'winter-eggs' formed from the substance of the Euglena-pellicle, as between gemmæ and the similar 'eggs' derived from Chlorococcus-heaps. But during the development of such bodies in the Euglena-pellicle, there was the same absence of an investing membrane in the early stages as had been noted in their formation from Chlorococcus

[1] About $\frac{1}{1500}''$ in diameter.

corpuscles—although the sharp limitation of the transformative change was now even more remarkable. It extended outwards for a certain distance only, so that those portions of the Euglenæ which came within the radius of the future 'egg' were decolourized and converted into a sort of fawn-coloured substance, whilst outlying portions of the same Euglenæ—of whatsoever size they might chance to be—still continued green and healthy-looking[1]. On one occasion I saw two large 'eggs'

[1] The first of these bodies was observed on January 5, 1872, and is now in my possession as a microscopical specimen. At that time, and whilst I was still quite unaware of its nature, the following rough entry was made concerning it in my note-book:—'A large, brownish, egg-like body, spherical and $\frac{1}{200}''$ in diameter, imbedded in the midst of Euglenæ and of a brownish granular matter. These circumferential Euglenæ blended into the egg-like body, so that several were seen, one-half of which was colourless and the other still green! On focussing through, it [the egg-like mass] seemed to have the appearance of being made up of a dense aggregation of decolourized Euglenæ containing colourless granules. Although this brownish and somewhat refractive mass was perfectly spherical, it was evident that no cyst-wall existed.' And then my impressions concerning the mass were thus stated:—'Metamorphic influences seemed to extend for a certain distance all round from a centre—so that, as above stated, the circumferential portion of the mass was composed (apparently) of Euglenæ which were partly decolourized, whilst those parts beyond the radius of the sphere were still coloured green.' Again, on February 20th, after I had become aware of the developmental destiny of such a body—from having observed all the stages undergone by many specimens—one of them was found actually in process of formation, concerning which the following notes were made:—'Light portion measuring $\frac{1}{333}''$; surrounding Euglenæ obviously metamorphosing, and fusing with central granular mass.' The situation of the body was carefully marked, and the portion of pellicle of which it formed part was transferred to a watch-glass. When examined again after twenty-four hours, my notes say:—'It had become a distinctly

forming in close proximity to one another, each of them being still without any trace of an investing membrane; and it so happened that a Euglena, lying between them where they approached each other most closely, had partly been included within each of the future 'eggs.' One-third was decolourized on each side, so that this Euglena actually entered into the formation of each of the 'eggs,' whilst the middle third of its substance, including the red eye-speck, remained as bright as it had ever been!

The 'eggs' of Euglena origin differed from those produced from Chlorococcus by being not at all villous, and by not possessing a double envelope; whilst they differed from the gemmæ of almost similar size, subsequently produced by the Hydatina, principally by reason of their more opaque appearance and browner colour. The 'gemmæ,' in fact, not only have a lighter and more evenly granular appearance when examined by the microscope, but are also easily distinguishable by the naked eye, owing to their white colour. The 'eggs' of Euglena origin seemed to undergo developmental changes more rapidly than those derived from Chloro-

ovoid egg, $\frac{1}{2885}''$ in breadth by $\frac{1}{2410}''$ in length, with sharply defined border and no remains of Euglenæ on its [upper] surface.' My avocations during the day have generally rendered it impossible for me to carry out attempts at development satisfactorily. Moreover, the changes 'in conditions' which are necessitated by any such attempts have generally been found, as above stated, to check or change the direction of future development. Even in this case, the egg-like body produced was much smaller than usual. The first body observed—appearing as a spherical mass—was probably a very large 'egg,' seen endwise.

coccus, though not quite so quickly as the gemmæ thrown off by adult Hydatinæ[1].

No more beautiful sight can be seen by the microscopist than one of these bright-green Euglena carpets, uniformly flecked with its carmine 'eye-specks,' and irregularly studded with the mysterious egg-like masses, in each of which inscrutable molecular changes are progressing, destined to terminate in the production of a beautiful specimen of one of the largest and most complex of the Rotifers. It is indeed a supreme though utterly inexplicable process of Synthetic Heterogenesis, which almost as much surpasses in marvellousness the origin of Ciliated Infusoria from aggregated Bacteria, as this latter process transcends that of the formation of the minutest speck of living matter from colloidal molecules[2]!

What fate, however, awaits the various kinds of Rotifers? The finer specimens of Brachion, Hydatina,

[1] In a few days, after some of the Hydatinæ produced from the pellicle have been hatched, other heterogenetic germs developing from the pellicle become abundantly intermixed with the large egg-like gemmæ thrown off from adult Hydatinæ. A little experience, however, will soon enable the observer easily to distinguish the former from the latter. The observations which revealed this mode of origin of Hydatinæ were made during the months of February and March in the present year. It was, however, long ago pointed out by Dujardin that specimens of *Hydatina senta* were almost invariably to be met with in association with Euglenæ, more especially during the spring months.

[2] See p. 262.

etc. soon produce large internal gemmæ (or so-called eggs), by means of which, instead of by fission, they rapidly multiply their kind. These internal gemmæ are, indeed, produced so soon in some cases that, according to Dr. Gros, 'dans quelques-uns de ces individus nées sans parents, on percevait déjà les œufs de leur future lignée.' The same writer, however, affirms that the great majority of the small Rotifers do not reproduce their kind[1]. Scarcely more than one out of a hundred is ever seen to contain anything like an 'egg' or embryo in its interior, although in these exceptional individuals gemmæ, capable of reproducing the individual, do make their appearance in the interior of what seems to be the first rudiments of an ovary. This process of internal gemmation as it occurs in some of the small Rotifers is, indeed, a process only slightly more advanced than that which also takes place, at times, amongst some of the Ciliated Infusoria. In fact, like the latter organisms (from which they are so frequently derived), small Rotifers are generally mere transitory forms, which, under favourable circumstances, have a tendency to undergo still higher transformations. Concerning such complete transformations Dr. Gros says[2]:—'Dans de certain circonstances, on voit le contenu de petits Rotatoires équivoques se résoudre dans la carapace en des ovules qui peuvent donner

[1] Dr. Gros, moreover, says that they rarely live more than eight days (loc. cit., pp. 308, 309).

[2] Loc. cit., pp. 430, 451, 309, Pl. O, figs. 7 and 8.

naissance à des Nématoides, qui chose curieuse, naîtront dans la carapace des petits Rotatoires conservés dans un vase, tandisque dans un autre vase les Rotatoires donneront d'autres produits infusoriels.'

The statement of the occurrence of such transformations will doubtless prove most surprising to the reader, but yet it can be capped and in a measure confirmed by others equally surprising[1].

Thus, it has already been stated that the smaller Rotifers, as well as some specimens of the larger varieties, frequently undergo a process of pangenesis when they have reached the term of their existence, whereby portions of their substance, becoming individualized, separate in the form of Actinophrys[2]. Such forms are generally very vigorous and most voracious, so that they rapidly increase in size, until they have acquired considerable dimensions. Having already[3] alluded to the fact, ascertained by Dr. Gros, that some of these organisms after a time retract their rays and become converted into specimens of the larger and more complex of the Rotifers, it now remains for us to add, that other apparently similar specimens of

[1] I have seen so many startling transformations myself, and have, moreover, been able to confirm Dr. Gros' observations in so many respects, that I see no reason for doubting these particular observations, more especially as they refer to masses of matter of a comparatively large size.

[2] See p. 484.

[3] See p. 505.

Actinophrys may, for some inexplicable reason, become converted into Nematoids instead of Rotifers[1]. Nay, more, other specimens of Actinophrys, similarly derived from the pangenesis of a Rotifer, may, by a slightly more round-about process, also give rise to specimens of the arachnidal Tardigrades[2].

According to Dr. Gros, this latter transformation takes place in the following manner:—A large specimen of Actinophrys gradually becomes spheroidal, whilst it retracts all its rays except one, by means of which the mass continues anchored during its subsequent stages. A succession of changes then takes place in its internal substance, accompanied by the appearance and disappearance of fatty-looking vesicles, whilst during this time the external membrane becomes more and more condensed and assumes an orange-brown colour. The internal vesicular mass is gradually converted into an active embryo, which in from fourteen to twenty-one days ruptures its enveloping membrane and appears as a large ciliated Planariole, visible by the naked eye. This organism, after increasing in size and leading a very active life for about ten days, gradually contracts and finally becomes encysted[3], and the mass thus formed

[1] If a Nematoid and a Rotifer may come from two apparently similar matrices, then the transformation of the Rotifer into the Nematoid, to which we have previously alluded, is only a very little more startling than the transformation of one form of Ciliated Infusorium into another, or of a Ciliated Infusorium into a small Rotifer.

[2] Loc. cit., p. 438.

[3] See loc. cit., p. 439; Pl. N, figs. 1–6.

in its turn, becomes transformed into an embryo, which immediately develops into one of the Tardigrades.

These facts concerning the transformations of the specimens of Actinophrys derived from Rotifers, may be paralleled by others which are generally in accordance with what we have hitherto been led to expect by the history of previous metamorphoses. In the first place, specimens of Actinophrys derived from quite a different source are also, according to Dr. Gros, capable of undergoing similar developments. Thus, some specimens directly derived from Euglenæ may, as we have already mentioned, be directly converted into Rotifers; whilst others, apparently similar, may be converted either into Tardigrades or into Nematoids[1]. And, secondly, these statements are borne out by others, based upon evidence of the most unmistakeable character, to the effect that Nematoids, Tardigrades, and the largest Rotifers may each of them be directly produced by the heterogenetic transformation of similar vegetal vesicles[2]. Dr. Gros says he has seen thousands of large rose-coloured Euglenæ gradually decolourize and become converted into encysted animal embryos, some of which produced large Rotifers, others Tardigrades, and others Nematoids. And the latter, though sexless at first, subsequently grew into well-developed males and females.

[1] Loc. cit., pp. 300, 309.

[2] In this respect, therefore, they are like all the inferior members of the series—they may either be the final outcome of a series of ascending developments, or they may be the products of a direct metamorphosis.

These metamorphoses, taking place as they do in masses of matter $\frac{1}{250}''$ or more in diameter, can be followed with the greatest ease; and, moreover, certain features are frequently presented which preclude all possibility of mistake with regard to the identity of the bodies undergoing transformation. Thus, it occasionally happens that one or two portions of the Euglenæ escape transformation, and remain within the cyst by the side of the embryo—just as portions of the embryonal spheres of Nitella occasionally escape transformation when these segment into Monads [1]. And at other times, phenomena have been observed which are still more capable of convincing even the most sceptical of those who have not themselves witnessed such transformations. Some of the encysted Euglenæ undergo a process of fission, and whilst still enclosed within the common cyst-like envelope, one of the bodies so produced may become rapidly decolourized and converted into an embryo Nematoid, whilst the twin product remains by its side as a still green or only very slightly decolourized Euglena (Fig. 94, *a*). Though at other times, as Dr. Gros says [2]:—'Une certaine série d'Euglènes se décolourait entièrement, comme à l'ordinaire et donnait

[1] See Fig. 79 *c*. At other times, however, the transformation into Monads takes place without any remainders; and so Dr. Gros says concerning these large Euglenæ which became transformed into Rotifers, Nematoids, and Tardigrades:—'Ce rebus de substance s'est présenté presque constamment cette année, tandis que, l'année dernière, l'Euglène se transformait de toutes pièces.'

[2] Loc. cit., p. 475.

un vigoureux vermicule.' And he adds:—'Ces choses incroyables peuvent être révoquées en doute, mais les sceptiques changeront peut être d'avis après une observation quotidienne de trois mois.' Every one who looks must not, however, necessarily expect to see

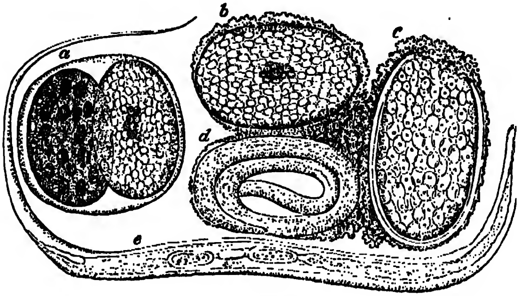


FIG. 94.

Origin of Nematoids from Euglenæ. (Gros.)

a. A large Euglena which after encystment has undergone fission, whilst one of the halves has become decolourized.
b. A Euglena which has become converted into a decolourized embryonic mass, leaving only a small coloured remainder.
c. Another decolourized mass, which, after undergoing certain changes, becomes converted into a young Nematoid, as at *d.*
e. A female specimen of the developed Nematoid three weeks old, in whose ovaries two partially-developed ova are seen.

these particular transformations. Dr. Gros says[1]:—'Cette génération directe de Nématoïdes ne s'était pas vue l'année dernière. Les dernières générations de Rotatoires seulement avaient donné des vers.'

Quite recently I have myself had the good fortune to

[1] Loc. cit., p. 477.

witness a direct origin of Nematoids from some of the thick-walled resting-spores of Vaucheria. A specimen of this plant had been growing beneath a bell-glass outside my window for more than a month, and portions of it had been repeatedly examined, though on none of these occasions had a single Nematoid been seen. But, wishing to observe the effects of a sudden alteration of conditions upon some of the organisms contained in the saucer, I took it into my study, and kept it (still beneath a bell-jar) in a part of the room away from the window—so that it was simultaneously exposed to a higher temperature and to a diminution of light. Four days afterwards the weed in many parts was found to be shrivelled and undergoing decolourization. It was evidently dying. On taking up a minute portion of the Vaucheria thus affected, I was much surprised to see three or four active Nematoids. And each minute portion subsequently examined was always found to yield from three to eight of these animals, which, on examination with higher powers, I at once recognized as forms similar to those to which Max Schultze had previously given the name Diplogaster[1], and of which I had described two or three new varieties[2]. These particular Nematoids now existed by thousands in the small saucer containing Vaucheria, in which not a single specimen had been seen four days before, or for a month previously. They were,

[1] A form which is figured in Carus's 'Icones Zootomicæ,' Tab. viii. fig. 1.

[2] 'Trans. of Linn. Soc.' vol. xxv. p. 116.

moreover, all young immature forms, in which the sexual organs, if present at all, were only partially developed. Whilst not a single adult female was seen, immature females and males were abundantly represented, and especially the latter. What then had been their mode of origin? A careful microscopical examination of almost any minute specimen of the weed soon made this quite obvious. In addition to the free forms, many embryo Nematoids were seen coiled up within ovoid cysts and lying amongst the filaments. Cysts were also seen containing a mere embryo mass, which either had or had not begun to undergo segmentation; whilst other cysts existed (of the same size or only a trifle smaller), which were spherical or ovoidal, and contained either green chlorophyll corpuscles or chlorophyll corpuscles in a state of decolourization (*a*)[1]. Other cysts existed in which the decolourized corpuscles had fused and become converted into a colourless embryonic mass containing a multitude of fatty-looking granules and globules, of different sizes (*b*)[2].

[1] The process of decolourization, too, was different from anything which I had previously seen. The green corpuscles gradually assumed fainter and fainter tints. and at last became colourless, without having passed through any of the customary intermediate tints. Similar processes, however, seem to have been observed by Dr. Gros, since the transformed vesicles, derived from Moss-sporangia, represented in Pl. J, figs. 4–7, seem to be remarkably similar to the Vaucheria spores, although instead of producing Nematoids they mostly gave rise to Rotifers.

[2] The transformation of chlorophyll into fatty products is easily explicable (see note 2, p. 425), and the evolutional tendencies of nitrogenized fatty substances has been before referred to (vol. i. p. 212, note 1).

It was, indeed, quite obvious that the green bodies between $\frac{1}{300}''$ and $\frac{1}{400}''$ of an inch in diameter were the thick-walled resting-spores of Vaucheria—some of them being still attached to the filament from which they had been produced[1]. It was obvious also that they gradually became decolourized and at the same time animalized, until at last the contained matter was resolved into a somewhat opaque and coarsely-granular, though more bulky, embryo mass. This mass subsequently contracted upon itself so as to leave a well-marked space between its outer surface and the wall of the cyst in which it was enclosed (*c*)[2]. It afterwards underwent the well-known process of segmentation, every stage of which was to be seen in different specimens (*d*).

Thousands of such bodies (about $\frac{1}{333}''$ long by $\frac{}{500}''$ broad) existed, and in addition to the facts already mentioned, proving the parental relationship existing between these thick-walled spores of Vaucheria and the embryo Nematoids, it is especially worthy of note that when the latter were first seen, not a single adult form was to be encountered. Though after about one month, those of the previous embryos which had

[1] These thick-walled spores are occasionally produced in large numbers upon the sides of the filaments by a process of 'conjugation' occurring between the contents of two contiguous tubular outgrowths.

[2] The space, however, seemed to be in part due to a still further dilatation of the cyst, the membrane of which had by this time become most appreciably thinner.

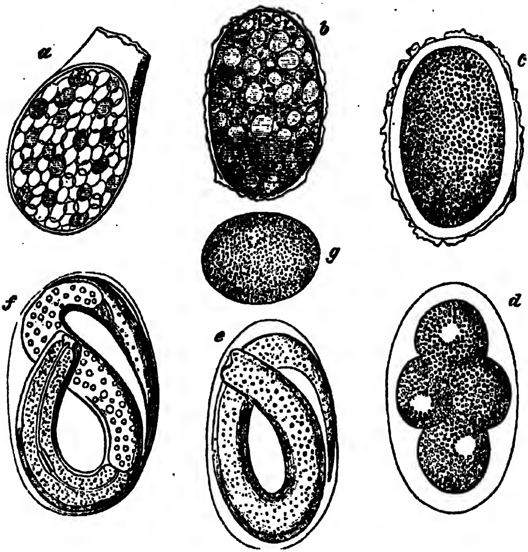


FIG. 95.

Development of Nematoids from Spores of Vaucheria. (× 400.)

a. Half-decolourized spore with portion of tube attached—some of chlorophyll corpuscles colourless, and others still green.

b. Similar body, wholly decolourized and swollen, resolved into a mass containing fatty-looking particles and globules.

c. Similar body in which animalized mass has become finely granular and somewhat smaller. (This and last surrounded by adherent granular débris.)

d. Showing early stage of segmentation of embryonic mass.

e. The mass subsequently becomes resolved into a worm-like embryo, which soon exhibits its first very slow movements.

f. Form assumed by the active embryo (*Diplogaster*) just before it ruptures its cyst.

g. Egg subsequently laid (after about a month) by the adult worm.

survived had attained the adult condition[1], and the females presented, as is usual with the free Nematoids, a few ova of relatively large size. The males were about $\frac{1}{18}''$ long by $\frac{1}{1000}''$ broad, and the females $\frac{1}{12}''$ long by $\frac{1}{500}''$ in breadth; whilst the largest egg seen, which was at the vulva and about to be expelled, measured $\frac{1}{600}''$ long by $\frac{1}{770}''$ broad—so that the ovum of this particular form of Nematoid was very much smaller than the heterogenetic matrix from which the parents had been derived[2]. The heterogenetic cysts of development, indeed, were so large that they could not by any possibility have been produced within such Nematoids as were developed from them.

This mode of development of the Nematoid is most interesting, and when compared with that of the Rotifer affords certain well-marked contrasts. In the first place, the process of decolourization took place in a different manner—it was more simple and not attended by the development of all those intermediate tints which usually appear when the resulting mass is about to shape itself into the form of a Rotifer. And in the second place, the well-known process of segmentation which occurs in the embryo mass of the future Nematoid

[1] Dr. Gros says that some of the specimens observed by him attained the adult condition in about three weeks. The time mentioned above, however, is much shorter than suffices for some of the homogenetically-produced free Nematoids. (See 'Philos. Trans.' 1866, p. 611.)

[2] The great superiority in size of the heterogenetic germ may account for the greater rapidity with which the large embryos so derived attain their adult condition (see p. 551).

is, so far as my experience goes, wholly unrepresented in the development of the Rotifer—here the embryo mass seems to shape itself more or less directly into the future organism, and a similar direct process of development without any antecedent stages of segmentation seems to obtain amongst the Tardigrades and the various forms of Mites. For although the Tardigrades represent the lowest terms of the Articulata, and therefore belong to a higher type—yet their grade of development is very low. So far as their mode of reproduction is concerned, they seem to be very closely allied to the large Rotifera: for the most part no distinct sexual forms exist, and only imperfect females are met with containing a rudimentary ovary (often without a distinct external duct), in which large gemmæ or so-called 'eggs' are formed. In the case of the Nematoids, however, embryos are produced from the heterogenetic matrices, which, though sexless at first, subsequently develop into what we are accustomed to call 'males' and 'females'—the latter possessing a double tubular ovary with a distinct external duct, in which a few large ova are generally to be seen [1].

The fact, therefore, that animals with such distinct and specific organs—and of different 'sexes' too—

[1] The typical form of this ovary is represented in Fig. 1 *a*. There is good reason for believing that in many of the Nematoids reproduction takes place by means of mere gemmæ, though in other cases bodies similarly produced are fecundated, i.e. stimulated by contact with certain elements secreted by the male.

should arise in this definite manner from the reproductive products of a plant, will doubtless seem to many to flavour more of fable than of fact. After the observations which have been detailed, however, we must accept the occurrence of such phenomena as established facts—just as we are compelled, and are now quite accustomed, unhesitatingly to believe in the reality of other equally inexplicable phenomena. When we are able really to explain the reason of the processes by which one minute vesicular mass of fatty and albuminoid particles develops into a man, another into a fish, and another into an insect, we may then with a little more show of reason think of rejecting other more or less similar facts because they are incomprehensible!

However true may be the views set forth in this chapter, it might reasonably be expected that others would give credence to them all the more readily if it could be shown that many facts long known—though difficult of explanation—were now more easily to be understood. Let us, therefore, briefly glance at a few of these old difficulties.

We have already alluded, on more than one occasion, to the general accordance between our views and the facts known concerning the succession of higher and higher organisms which gradually replace one another in infusions[1]. And yet, on the old hypothesis, there is the greatest difficulty in accounting for these pheno-

[1] See p. 502.

mena. But a much more marked discrepancy between facts and generally received theories exists in reference to other phenomena. Thus, when speaking of Rotifers, Pritchard says, in his 'Infusoria' (p. 655), 'One remarkable circumstance must be borne in mind by the animalcule hunter. If he happens to remember a pond where some rare species abounded last year, let him not again turn thither in search of it, as the chances will not be in his favour. These creatures rarely exist in the same water during two successive years. The reasons for this are not easily ascertainable. The remark is equally applicable to Volvox and the Desmidiæ. The search will be most productive if prosecuted on new ground.' Now this variability in the habitat of the rarer kinds of Infusoria, incompatible as it is with received notions concerning 'winter-eggs' and 'resting-spores,' is thoroughly harmonious with all that we have said concerning the life-history of many of these forms and the extreme variability at different times in the products of heterogenetic transformation—peculiarities which were long ago dwelt upon by Dr. Gros.

Again, the extreme prevalence and almost universal distribution of certain common forms of Rotifers, Tardigrades, and Nematoids[1], will become quite inexplicable to those who disbelieve in the occurrence of heterogenetic transformations—in the face of our present

[1] Each of these three forms of life having the peculiarity that they multiply their kind by notably large gemmæ or eggs.

knowledge concerning the distribution of large germs or ova throughout the atmosphere. The great prevalence of such forms of life is, however, easily explicable in accordance with the possibility of their origin by more or less rapid series of ascending developments, or by processes of direct transformation of vegetal vesicles.

Thus it is, as Pritchard says[1], a well-known fact that some Rotifers 'are common wherever water has remained for a little time without disturbance—in cisterns, depressions in the gutters of houses, saucers of flower-pots, and similar situations.' And again, in a memoir published in 1865 'On the Anatomy and Physiology of Nematoids[2],' I stated that 'an examination of tufts of moss from the roofs of houses or from old walls, as well as of specimens of the yellow lichen (*Parmelia parietina*) from the same situations, has invariably revealed to me three principal kinds of animal-occupants—specimens of Rotifers, of peculiarly slow-moving arachnidal Tardigrada, and two or three different kinds of Anguillulidæ. Precisely the same varieties of animal life are spoken of as existing in the tufts of moss examined by Spallanzani, and also in those which were experimented upon by Andral and Gavarret. Moreover, I have found specimens of lichen brought from Sweden tenanted by just the same types.' Now those who believe that such forms of life are produced only from pre-existing eggs of similar forms, must find it

[1] 'Infusoria,' p. 653.   [2] 'Philos. Trans.' 1866, p. 617.

extremely difficult to understand their universal distribution and co-existence in almost every tuft of moss or lichen growing in all sorts of remote situations, seeing that no one has ever recognized amongst filtrations or depositions from the atmosphere any of the large eggs by which each of these forms usually reproduce their like. The difficulty is, indeed, similar in kind, though very much greater than that to which we have already referred in connection with the development of the common edible Mushroom in suitably prepared beds. The very small size of the fungus-germs did afford a sort of excuse, though this becomes wholly unavailable when we have to do with the large eggs of Rotifers, Tardigrades, and Nematoids[1]. Nobody pretends that these are universally

[1] See p. 433. It is also most important to note that Nematoids may often be obtained at will—and this almost as readily as Mushrooms—by a process which seems to be just as independent of ordinary eggs as the other is of ordinary spores. I first learned about nine years ago from some of the writings of Dr. Schneider (Müller's 'Archiv.' 1858 and 1860) that a form of free Nematoid, named by him *Pelodytes*, was generally to be obtained by burying a few small pieces of flesh in a little damp earth. Soon afterwards, whilst living at Broadmoor in Berkshire, I tried this experiment—having carefully examined the beef beforehand to see whether it contained any encysted Nematoids or their ova. But having found none, small portions of it were buried in a small quantity of earth contained in a gallipot. The earth had been frequently examined before without revealing a single Nematoid. After three weeks this earth was found to be absolutely swarming with two kinds of Nematoids—quite different from any forms which I had previously seen, although I had been seeking them for more than two years previously in all sorts of situations. I am, unfortunately, unable to say what were the precise stages in the evolution of these Nematoids, since the mixture

distributed through the atmosphere, and, indeed, he would be a rash man who would promise to find a single one of such easily recognizable bodies amongst the products of filtration from the atmosphere, obtained by a week's work. The Panspermic hypothesis, indeed, breaks down even more utterly with regard to the germs of these comparatively high organisms than it does with reference to the distribution of Bacteria (pp. 6, 7). But although the almost universal distribution of Rotifers, Tardigrades, and Nematoids cannot be at all accounted for by those who rely simply upon Homogenesis and the supposed universal distribution of the germs of these and other organisms, all the facts may be easily explained by an acceptance of the reality of such heterogenetic processes as we have already described. Heterogenesis is, in fact, the necessary appanage of Archebiosis—so that the reality of the one process almost necessitates the reality of the other.

The importance of an adequate consideration of

was not systematically examined at the time. I was much impressed, however, by the facts which were then wholly inexplicable to me—and as such I recollect mentioning them soon after to two or three eminent biologists. These observations were made very shortly before I left Broadmoor, and since that period the great pressure of other kinds of work has always prevented my following up this observation by new experiments. These, however, had been contemplated, and I had intended to resort to comparative trials of different kinds of meat mixed with similar specimens of earth and water, and at the same time to subject these mixtures to careful and daily microscopical examinations. Experiments and observations of this kind, which still have to be made, would, I have no doubt, soon lead to most important results.

these processes of Heterogenesis on the part of the biologist is enormous. The knowledge obtained from the study of such phenomena must suffice to throw far more than a glimmer of light upon some of the most obscure of zoological and botanical problems. The question, however, which now most naturally presses for solution is—Where do such processes end? At what grades of complexity in the animal and the vegetal series will Heterogenesis cease to appear as an occasional mode of origin for totally distinct specific forms?

So far as animals are concerned, we can even now say that evidence exists tending to show that some members of every one of the groups belonging to the class **Scolecida** may be produced by a process of Heterogenesis. Evidence has been abundantly brought forward in this chapter concerning the occurrence of such modes of origin amongst the *Rotifera* and amongst the order *Nematoidea*. Moreover, observations have already been recorded by Dr. Gros in other memoirs than those to which we have alluded, tending to show that specimens of the orders *Acanthocephala*, *Tæniada*, and *Trematoda* may also be produced by processes of Heterogenesis; whilst I have myself very strong reasons for believing that one of the *Turbellariæ*—the strange but highly interesting Chætonotus—may also arise by a process of Heterogenesis taking place within the filaments of Nitella. But even this is not all. As I have previously stated, there are the positive observations of Dr. Gros concerning similar modes of

origin of some Tardigrades, and I have already in my possession much convincing evidence, derived from personal observation, that some water Mites or Acari, and also the ciliated embryos of Naïdes, may be produced by a direct transformation of masses composed of the protoplasm and chlorophyll of Nitella.

Thus, in addition to representatives of each of the groups belonging to the class **Scolecida**, there is already reason to believe that we may at once attain, by heterogenetic transformations of the most startling character, to some of the lowest **Annelida** and also to some of the lowest of the **Arthropoda**. And whilst the actual origin of the representatives of these great groups of the Animal Kingdom would thus be laid bare by researches conducted upon fresh-water forms of life, marine products remain as yet wholly unexplored from this point of view—and consequently whole mines of wealth are probably awaiting the diligent student in these fields of research. On the other hand, how much remains to be ascertained concerning some of the lower representatives of the Vegetable Kingdom! To what extent, if at all, is a heterogenetic mode of origin possible amongst Ferns and Club-mosses? To what extent does it prevail amongst Mosses, Liverworts, and other allied Cryptogams? These are some of the numerous problems which remain to be decided by future workers.

CHAPTER XXIII.

INDIVIDUALS, EPHEMEROMORPHS, AND SPECIES.

Individuality. Different views concerning. Modes of Origin of Independent Units. Views about 'Species.' Recent Modifications. Modes of Reproduction. Dawn of Sexual Process Blending of Sexual and Asexual Processes. Views of Prof. Huxley and Dr. Carpenter. These not in accordance with doctrines of Evolution. Other objections. Nature of Lowest Forms of Life. Their correlation with one another. Ephemeromorphs. Their different Modes of Origin. Light thrown upon 'Alternate Generation.' Principal Varieties of. Bearing of facts upon Nature of Individuals and Species. Explanation of apparent Anomalies. Transmutations of Species. Mr. Darwin and 'Natural Selection.' Two meanings of Term. These not always distinguished. Mr. Herbert Spencer's Views. Influence of Change in External Conditions. Direct and Indirect actions of. Instances cited by Mr. Darwin. Internal Causes of Change. Great difference of Views concerning. Mr. Herbert Spencer's Doctrines. Opposing Nature of, on this Subject. Undue importance attached to External Influences. Mr. Darwin not an advocate of 'Progressive Development.' Slight influence of External Conditions over Lower Organisms. Their Forms more dependent upon Molecular Polarities. Instances of 'Spontaneous' Variation. Convertibility of the Nectarine and the Peach. Origin of black-shouldered Peacock. Importance of such Transformations. Influences which produce and modify the Forms of Organisms. Author's Views as compared with those of Mr. Darwin and Mr. Herbert Spencer.

WHILST the various modes of origin of living things and their subsequent metamorphoses are subjects of surpassing interest, the true meaning or

importance of the processes themselves cannot be adequately realized unless we have arrived at clear conceptions as to the respective notions which should be attached to the words 'individual' and 'species.'

It might, and doubtless will, be thought by many, that whatever conflicting opinions may be entertained of the precise meaning of the latter term, no similar doubts could prevail concerning the significance of the word 'individual.' Such, however, is not the case. Attempts, based upon much good and legitimate reasoning, have been made by Dr. Carpenter[1] and Prof. Huxley[2] to distinguish between the ordinary meaning of this word and the special meaning in which it is thought desirable that it should be understood by biologists. So many subsequent writers have, moreover, accepted the views and phraseology which they proposed, that the words 'phytoid' and 'zooid' are now frequently to be met with in the pages of botanical and zoological writers. Hence it is all the more incumbent that we should enquire into the propriety of continuing to maintain the distinctions which these words were intended to suggest—that we should endeavour, in fact, to ascertain whether such distinctions are still required, or whether they are even compatible with our present state of knowledge.

[1] 'Brit. and For. Med. Chir. Rev.' vol. i. (1848) p. 183, and vol. iv. (1849) p. 436.

[2] 'Philos. Trans.' 1851, pp. 576-579; and 'Ann. of Nat. Hist.,' 2nd ser., vol. ix. p. 505.

It must be recollected that difficulties always beset our path when we attempt to define or separate by hard and fast lines, things which—being evolved from one another—are produced by a gradual process of modification. It most frequently happens that no definition can be framed which will not involve some contradictions. All that remains, therefore, in such a case is to strive to select that meaning for any word in question which is most congruous with its usual meaning, and the adoption of which will involve the fewest contradictions.

Without now going into the reasons which have been urged on both sides—and which have been very fairly set forth by Dr. Carpenter[1]—we will simply state that we now agree with Mr. Herbert Spencer[2] in thinking the conditions above mentioned to be best fulfilled by considering that 'a biological individual is any concrete whole, having a structure which enables it, when placed in appropriate conditions, to continually adjust its internal relations to external relations, so as to maintain the equilibrium of its functions.' And then, as Mr. Spencer adds:—'In pursuance of this conception, we must consider as individuals, all those wholly or partially independent organized masses which arise by multicentral and multiaxial development, that is either continuous or discontinuous. We must accord the title to each separate aphis,

[1] 'Principles of Comp. Physiol.,' 4th ed., 1854, pp. 523-529.
[2] 'Principles of Biology,' vol. i. p. 207.

each polype of a polypidom, each bud or shoot of a flowering plant, whether it detaches itself as a bulbel or remains attached as a branch.' All these are 'capable of independently carrying on that continuous adjustment of inner to outer relations which constitutes Life,' and therefore we may look upon them as biological individuals—even though some of them do not actually exist as concrete wholes which are entirely separate. Individuals, in fact, are at first single and separate, though they may give origin to others which either remain in a state of aggregation or which may separate from one another, according as their growth is continuous or discontinuous. This mode of viewing the question seems to me to be the one which involves the fewest inconsistencies, though in order to make this position clear to others, we must enter upon certain preliminary discussions.

The modes by which separate individuals have been seen to arise are so various that they will be best understood in relation to one another by throwing them into a tabular form [1]. We shall thus be enabled to bring out the differences and resemblances between the several methods more clearly and briefly than we could otherwise do.

[1] The Homogenetic modes of origin have been already referred to (see vol. i. pp. 232–235); they must now be classified with the Heterogenetic modes of origin.

Modes of Origin of Independent Living Units.

Evolution.	*a.* In simple Saline Solutions (?)—*Torulæ, Bacteria*, &c.			**Archebiosis.**	
	b. In Colloidal Solutions.	1. Ordinary Macerations or Infusions—*Bacteria, Torulæ, Protococci*, &c.			
		2. Forming part of one of the higher Organisms.	*α.* In Health—*Leucocytes*. *β.* In some Diseases—*Bacteria*, &c.		
	c. By molecular re-arrangements in existing 'not living' aggregates, e.g. Milk globules and Amylaceous granules in tissues of Plants into *Fungus*-germs.				
Fusion.	*a.* Heterogenetic.	Of Bacteria to produce *Fungus*-germs, *Amœbæ Monads*, or Ciliated Infusoria. Of Chlorophyll Corpuscles, &c. in Algæ or Characeæ with production of *Actinophrys, Amœbæ*, Ciliated Infusoria, &c. Of Algoid Corpuscles to produce Rotifers.		**Biocrasis.**	Heterogenetic and Homogenetic.
	b. Homogenetic.	Of two *Amœbæ*, two *Gregarinæ*, &c. Formation of Embryo in *Buccinium* and *Purpura*. Formation of some Winter-Eggs in *Rotiferæ*, &c. Formation of Ovum in Nematoids, Insects, &c. by fusion of granules and mucoid matter.			
Reproduction.	*a.* Heterogenetic.	Segmentation and Gemmation.	1. Multiple simultaneous fission. Products separate—*Amœbæ* into *Fungus*-germs, &c. Products having as associated existence—*Palmoglœa* from Moss-leaf. 2. External gemmation from Algæ producing *Desmids*, or *Diatoms*.	**Biodiæresis.**	Heterogenetic.
		Rejuvenescence.	Leading to ascending developmental changes of variable nature in *Euglenæ, Amœbæ, Actinophrys, Vorticellæ*, &c. Leading to other ascending developmental changes in Chlorophyll corpuscles of Algæ and Characeæ producing *Monads, Amœbæ, Diatoms, Desmids, Algæ, Euglenæ*, &c.	**Biocœnosis.**	
		Secondary Evolution.	Origin of *Bacteria* and *Fungus*-germs within pre-existing protoplasm constituting animal or vegetable cells.	**Bioparadosis.**	
	b. Homogenetic.	Fission, Segmentation, or Gemmation.	Fission of *Bacteria, Protococci, Amœbæ*, &c. Segmentation in chambers of Algæ and Fungi producing zoospores or spores. Gemmation in *Torulæ, Hydræ*, &c.	**Biodiæresis.**	Homogenetic.
		Rejuvenescence.	Formation of 'resting spore' in *Œdogonium*, &c. Also formation and fusion of two such bodies, as in *Zygnemeacæ*.	**Biocœnosis.**	
		Secondary Evolution.	Formation of 'nucleus' in plastides and of 'ovum' in *Kolpodæ*, &c. Mode of origin of spermatozoa and pollen. Mode of origin of ovule in *Phanerogamia* and of rudiments of ovum in *Mammalia*, &c.	**Bioparadosis.**	

Of these five ultimate methods, two or more are often associated in giving origin to a single new individual, though at times one alone may be instrumental. They are all more or less closely related to one another, and are immediately dependent upon the fundamental properties of living matter. Some of the processes are, moreover, quite similar to those which go on in not-living matter. Elements combine, acids and bases combine; and similarly, colloidal molecules may unite and undergo rearrangements, so as to give origin to units of living matter (Archebiosis). Again, as we have previously pointed out, Biocrasis is but a further stage of the process which occurs in Archebiosis; whilst Bioparadosis (or secondary evolution) is also most closely allied to the same process. The occurrence of Biocænosis is dependent upon the inherent complexity and mobility of the ultimate molecules of living matter; whilst, in their actual nature, such processes are strictly comparable with the phenomena of allotropism and isomerism as they occur amongst not-living elements and compounds. Biodiæresis (or discontinuous growth) is essentially dependent upon a higher manifestation of the same property—constituting as it does one of the most distinctive characteristics of living matter.

The simple individuals which arise by any of these various methods may, when their 'discontinuous growth' is rapid—owing to the occurrence of processes of fission or gemmation—give rise to many small and separate individuals. Whilst others, when they do not undergo

such processes, develop into more complex individualities, which are produced by the gradual differentiation of new individuals as growth progresses, and the continued cohesion of these new and old individuals into a single mass, assuming the form of this or that organism [1].

But what meaning are we to attach to the word 'species'? An ability to produce their like displayed by the individuals of successive generations of similar forms, combined with a changeability not going beyond certain narrow limits, were the two fundamental ideas formerly connoted by the word. An original representative of each species was for a long time, and very generally, supposed to have been specially created with the power of perpetuating itself by reproduction. Different specific forms were, therefore, not supposed to be derived one from another by any gradual process of change, but to have been created in the form in which they are seen to exist. Now, however, thanks more especially to the writings of Mr. Darwin, this hypothesis is no longer received as an established truth by a large and increasing number of naturalists, whilst it is wholly rejected by very many of the most eminent biologists of the present day. The old 'special-creation-hypothesis'

[1] Some individualized portions of such a mass may from time to time separate either as agamic buds or as 'seeds' and 'ova'—and each of such separated portions of the organism are themselves capable of undergoing more or less similar processes of development.

is, indeed, now looked upon as a well-nigh exploded notion—as one which, whilst favoured by no valid evidence whatsoever, is absolutely opposed to much general and special knowledge of the highest worth[1]. Doctrines of Evolution are, therefore, becoming more and more popular amongst biologists, and the mutability of species is now very generally proclaimed.

If, then, the old word 'species' is to be retained, its connotation of immutability must be understood to have been lopped off; so that 'ability to reproduce their like through successive generations' would remain as the distinguishing characteristic of those assemblages of similar individuals which are usually grouped under the word 'species.' The term would seem, then, to be applicable to any assemblage of living things, the members of which were capable, through many generations, of giving rise to other similar forms by a process of reproduction.

But, it may be asked, is it or is it not of consequence how this process of multiplication is brought about? Processes of fission or of gemmation, variously combined, constitute the sole modes of reproduction occurring amongst all that vast assemblage of organic forms which were provisionally included by Professor Haeckel in the kingdom Protista. Amongst the vegetal forms, it is true, processes of 'conjugation' are also met with—representing the first dawnings of the more com-

[1] For an excellent summary of the evidence on this subject, see Mr. Herbert Spencer's 'Principles of Biology,' vol. i. pp. 333–364.

plex 'sexual' method of reproduction. And, on the other hand, amongst the more distinctly animalized specimens of the Protista, processes of fission and of external gemmation gradually become mingled with processes of internal gemmation or budding, as in the Ciliated Infusoria.

Whilst, outside the pale of the Protistic kingdom, we find amongst Rotifers that fission and external gemmation for a time disappear, so as to give place to a more frequent reproduction by internal buds or 'gemmæ,' formed within a rudimentary ovarium. And, similarly, amongst the Tardigrades, the mode of reproduction seems to be also for the most part of the asexual variety—'gemmæ' resembling ova being generally produced within an ovarium, a little more distinct than that which is met with amongst Rotifers. But the power of homogenetic reproduction in these remarkable organisms is not limited to the rudimentary generative organ. Dr. Gros tells us that the dead Tardigrade may ultimately be resolved into specimens of Actinophrys, Peranemata, or Arcellinæ by processes of Pangenesis in every way similar to those occurring amongst Rotifers[1]. And whilst such heterogenetic products of the Tardigrade may, at different times, be either all of one kind or variously intermixed, it occasionally happens that two or three of the spherical masses into which the body-substance becomes resolved develop into young Tardigrades—even when the re-

[1] See pp. 483-486.

maining portions take on the forms of Actinophrys, Peranemata, or Arcellinæ. In other cases still, where the dying Tardigrade is perhaps placed under more favourable conditions, its whole internal substance, after a process of fusion and molecular elaboration, divides into large germ-masses (Fig. 96, *a*), and these

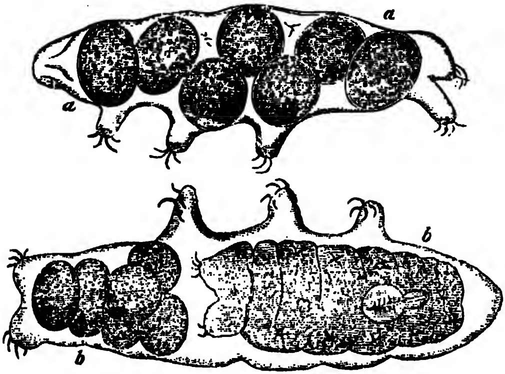


Fig. 96.

Homogenetic Pangenesis in Tardigrades. (Gros.)

a. Seven large germs, into which the total internal substance of the parent has become resolved—each of them being capable of developing into a Tardigrade.

b. A variety of the same process, in which a very large mass rapidly develops into a very large embryo. Other embryos seen in a much earlier stage.

directly develop into young Tardigrades, which, in about ten days, show their first independent movements. The number of young thus produced is very variable. There may be as many as twelve or fourteen formed within the same parent, though at other times the number is smaller, and one or two of the embryos

may be very much larger than the rest. For it sometimes happens that a mass of the body-substance which would ordinarily divide into three or four germs undergoes organization as a whole, so as to produce a single Tardigrade (*b*)[1]. Such a gigantic specimen is generally remarkable for its superior vigour and for the greater rapidity with which its developmental changes are achieved. In this respect it leaves its more diminutive relatives far behind. Thus it is seen that whilst Tardigrades may undergo all the processes of heterogenetic Pangenesis which are to be encountered amongst Rotifers, they also stand, so far as we know, alone, from the fact that they are capable of undergoing this remarkable process of homogenetic Pangenesis, and because, even in the same animal, we may see how imperceptibly heterogenesis may give place to homogenesis[2]. It is not, however, until we reach other animals, such as Acari and Nematoids, that well attested males and females are, as a rule, encountered. Although even amongst some of them it would seem that buds capable of developing into young Acari or young Nematoids, as the case may be, are still occasionally produced within the reproductive organs of the female quite irrespectively of any male influence.

[1] Loc. cit., p. 442.

[2] Although dead Rotifers do not resolve themselves into masses of matter which are capable of developing directly into similar Rotifers, yet, as Dr. Gros has pointed out, the specimens of Actinophrys or other forms which they do yield by Pangenesis, very frequently develop into Rotifers (though often of a different kind) after a previous existence in one or more intermediate states (see p. 505).

Thus the asexual modes of reproduction which exclusively prevail amongst the multitudinous groups of lower organisms included amongst the Protista, pass, by the most easy gradations, into the simplest kinds of sexual generation; and these simpler modes of sexual reproduction gradually give place to more specialized processes of the same kind. We find, moreover, that individuals which have been produced by an asexual process are often precisely similar to others that are the products of fertilized germs. We find that the asexual methods of reproduction predominate amongst some creatures which are, nevertheless, occasionally capable of multiplication by the higher sexual method. And, lastly, we find even in the highest organisms which habitually multiply themselves by the sexual process, an asexual mode of multiplication occasionally taking place. We have examples of this latter class of cases in the 'Parthenogenesis' which occurs in some of the Lepidoptera and other highest types of insect life; and also in the fission of the early embryonic mass which seems occasionally to take place during the origin of any of the forms of the vertebrate series—not excepting Man himself[1].

The gradual interblending and differentiation of the asexual and of the sexual processes may be seen from the following synoptical table, in which an attempt has been made to classify the best known modes of multiplication or reproduction which occur amongst living things:—

[1] Dr. Carpenter's 'Comparative Physiology,' 4th ed., pp. 480 and 580.

Now, in spite of such facts as are shown in the preceding table, Dr. Carpenter[1] and Prof. Huxley have argued that the product of a single fertilized germ should, in all cases, be considered to constitute a single biological 'individual'—even though such product may, in the course of its development, have given rise by fission or gemmation to a whole multitude of separate individuals, in the ordinary sense of the term. They have proposed, moreover, that the separate constituents of this biological individual should be designated by the term 'phytoid' or 'zooid,' according as we have to do with a plant or with an animal form.

The objections to this view of the case now appear to be many and insuperable. Thus, the view seems based upon the supposition that a sexual mode of generation necessarily exists amongst all forms of life. And yet the notion that the earliest living things would begin to multiply by a sexual process of generation is certainly not implied by the doctrine of Evolution. We can, however, much more easily understand how such a notion arose and was generally accepted whilst the 'special-creation-hypothesis' was still unrejected, and when our knowledge concerning the lower forms of life was of a more limited nature. Then older naturalists invariably looked about for a sexual method of reproduction, as a process essentially necessary, just as others have been led to seek, and to fancy they have found, even

[1] See Dr. Carpenter's 'Comparative Physiology,' p. 528.

in the simplest organisms, all those various specialized parts with which they have become familiar in higher organisms! The venerable Ehrenberg was not satisfied unless he could find in a ciliated Infusorium, a mouth, esophagus, stomach, and anus; together with heart and circulatory system, as well as male and female sexual organs. Whilst we may smile now at the simplicity which dictated all these expectations, it must be confessed that the postulation of the existence of a sexual process in all organisms, and the distinction between true and false processes of generation which have been consequently advanced, are founded upon similarly obsolete points of view. So that if a sexual process of multiplication did exist from the first, it would be a great difficulty in the way of those doctrines of Evolution to which, in other respects, Prof. Huxley and Dr. Carpenter have for many years given in their adhesion [1].

[1] There can be little doubt that the process of 'conjugation' supplies us with one of the first steps which gradually lead on in the direction of more specialized modes of sexual reproduction. But why, it may be asked, should the matter within two contiguous cells of a Spirogyra ever tend to fuse in this manner? One can only suppose that the matter within such cells must have assumed some 'polar' condition in which it becomes capable of exercising an attractive influence upon other more or less similar matter, and of being acted upon in turn. It is, therefore, most interesting to find that undoubted attractions and repulsions do occur amongst higher plants—even although in them such movements towards or from one another have nothing to do with the process of reproduction. Thus Mr. A. Henry has pointed out ('Gardener's Chronicle,' Dec. 9, 1871) that certain climbing plants display a partiality for plants of some other species by stretching out their tendrils or branches

But, as our table indicates, processes of agamic generation are alone met with amongst myriads of the lowest forms of life, which, hitherto, have not only been regarded as distinct 'species,' but have been ranged under different kingdoms, classes, orders, etc.

And the table also shows that similar agamic processes of fission may occasionally take place in the embryos, even, of vertebrate animals. So that if Prof. Huxley's and Dr. Carpenter's notions were to be adopted, we might be compelled to deny the attribute of individuality to each of the products of a twin birth, because the two may have originated by a process of fission in the early embryo: the two persons would in such a case be 'zooids,' or component halves of one individual, however repugnant such a conclusion might be to our ordinary conceptions. For if all the products of a fertilized germ are to be considered as one 'individual,' it would of course make no difference to those who thought it best to adopt such a nomenclature, whether the germ or embryo divided in its earliest stages, or whether it multiplied by a process of budding at some later period of its development. In each case the products, however numerous, would have to be regarded as constituting only one biological 'individual.' Fortunately, however, for those who might have

so as to come into contact with them; whilst towards other species the same climbing plants seem to display just as marked an aversion. They avoid one another and never touch, whilst running up the same wall side by side.

been much perturbed by this aspect of the question, the view is, as we have already pointed out, undesirable on other grounds. Whilst it is not adopted by Mr. Herbert Spencer, it is also discountenanced by Prof. Allen Thomson, who said[1], even in 1855, in his valuable article on the 'Ovum':—'It seems to require a greater departure from the ordinary signification of a common term than is warranted by our present imperfect knowledge of the phenomena, arbitrarily to determine to regard merely as one individual all those bodies which may be formed by a non-sexual process from the product of a single ovum, notwithstanding the great variations in their structure and mode of life, and the complete separation and apparent independence to which they may attain.'

It should not be forgotten, in fact, that the word 'individual' is a general name of the widest applicability, whilst 'species' is an abstract name—corresponding to no external reality—which is, therefore, capable of receiving an amended signification, without much inconvenience, whenever the progress of science may demand any such change. The connotation of the word 'species' has indeed already been unmistakeably modified in the minds of many persons, even within this generation; and we think it will be found that all the difficulties experienced by those naturalists who thought it necessary to alter the meaning of the word 'individual,' will be cleared away at less cost,

[1] 'Cyclop. of Anat. and Phys.' (Supplement), vol. v. p. 39.

and also much more effectually, by some further limitations in respect to the meaning of the word 'species,' such as the present state of our knowledge now renders absolutely necessary.

Must we suppose that all the forms of Life which are capable of reproducing their like or of 'breeding true' through successive generations, whether by a sexual or by an asexual process, are to be considered to belong to distinct 'species'? Although this is the view to which our previous remarks seemed to tend, we must not hastily commit ourselves to any such conception.

We find living matter coming into existence *de novo*, and possessed of a marvellous plasticity, so that different parts of it may, in more or less rapid succession, assume now one now another of a countless series of organic forms. During each of these periods also we find the several forms multiplying themselves by processes of fission or gemmation, and all the products of such multiplication capable of undergoing similar sets of changes—the nature of which always vary according to the precise, though unknown, molecular qualities of the different kinds of living matter and the conditions to which they are subjected. The products of a single stock may, moreover, display a considerable amount of diversity, because the precise molecular composition of the matter is so readily altered, and because each of these alterations involves

a new mode of moving equilibrium which may of necessity entail changes in form and structure.

We have, therefore, primordial specks of living matter assuming the forms of Bacteria, of Torulæ, and of the simplest corpuscular Algæ, all of which may multiply their kind indefinitely; we have these organisms at last taking on more continuous modes of growth, and thus unfolding into the most varied forms of simple Fungi, and also into filamentous or thalloid Algæ: whilst the latter, according to Dr. Braxton Hicks and others, may, in their turn, undergo modifications whereby they give rise to simple Lichens or Mosses—all of which also possess the power of self-multiplication. At other times, however, instead of witnessing the gradual unfolding of simple living units into higher forms, we may see changes take place in aggregations of the simplest units, which, in different cases, may terminate in the evolution of large Fungus-germs, of Amœbæ, of Monads, of Ciliated Infusoria, or even of Rotifers and Nematoids. These various animal forms—each of which is capable, by one or other method, of multiplying its own kind—are also to a very great extent mutually convertible into one another, and, in addition, the several forms are capable of being derived more or less directly from portions of vegetal matter thrown off from one of the Algæ, Lichens, or Mosses—although other portions of similar matter may become converted into Desmids or Diatoms.

Thus amongst the lower forms of life the correlation

is seen to be so intimate—the transitions are so rapid and irregular in their actual course on different occasions—that we cannot now look upon them as representing distinct 'species': although each of them may be perfectly capable of 'breeding true' on one or more occasions. Neither do the several forms occur in regularly recurring groups, so as to enable us to say that such and such varieties are mere developmental stages of one and the same ultimate form[1]. It is, therefore, on this account, and because nothing answering to those regular assemblages of definitely recurring forms which we include under the word 'species' exists amongst them, that I propose, henceforth, to speak of such evanescent and transitory organisms as 'Ephemeromorphs.' In the main, they are forms of Life whose motto might fitly enough be found in these words from Ovid:—

> 'Corpora vertuntur; nec quod fuimusve, sumusve,
> Cras erimus.'

Some of them, however, are organisms which may persist in the same condition for a very considerable period. This, for instance, is notably the case with certain varieties of Lichen which often live for a very long period[2]. But in other cases the forms only appear to

[1] Although this is the case occasionally with some forms—as when some few out of countless multitudes of Bacteria develop through Vibrio and Leptothrix forms into Fungi. Here it stops, however, for there is no normal production of Bacteria from the simple Fungi.

[2] Thus the Rev. M. J. Berkeley says:—'Some of the large patches of *Parmelia*, which occur on rocks, are of very great age. Patches of such

persist. That is to say, whilst they are individually short-lived, and are most prone to give birth, at different times, to other organisms of the most varied nature, they are also exceedingly apt to recur, quite independently, just as certain crystalline forms are apt to recur, when crystallizable compounds separate from the state of solution, at different periods.

Ephemeromorphs and 'Alternate Generation.'

The studies which have revealed some of the numerous and complex relationships existing between the multitudinous varieties of 'Ephemeromorphs,' have been of cardinal importance for the science of Biology. They have taught us, not only that living matter is formable *de novo*, but that it possesses inherent tendencies of such a nature as to make it prone to undergo variations in constitution, directly leading to variation in external form; that transitions are always easy in these early stages from the simpler vegetal to the more complex animal modes of growth, and *vice versâ*; and that, in the main, when placed under favourable conditions, the different forms tend to

Lichens as *Lecidea geographica* probably date from almost fabulous periods, and even small patches are often of considerable age. I have myself watched individuals for twenty-five years, which are now much in the same condition as they were when they first attracted my notice. Plants which endure without injury such extremes of temperature, and conditions of the hygrometer, would seem, *à priori*, to be likely to have great powers of longevity.' ('Introd. to Crypt. Bot.,' p. 418.)

become more and more complex. But in consequence of those laws of 'polarity,' upon which the form of the simple organism depends, almost any portions of such organisms, when once they are separated by fission or gemmation, grow more or less immediately into similar organic forms [1]. They 'breed true' in all their stages—just as a separated fragment of a crystal, when it remains immersed in the mother-liquor, will form the starting-point of another similar crystal. So that where, in the case of the ephemeromorph, the parent form has been attained by a continuous process of growth and development, we find that the spore or 'gemma' which is ultimately thrown off tends again to go through a similar series of changes. This may be seen in the development of a Fungus-conidium or spore into a form similar to that from which it had separated But where a succession of forms has been produced, one out of another, by the occurrence of several successive, though 'accidental,' heterogenetic transformations,—any portion cast off from the ultimate form is apt usually at once to develop into an organism similar to that ultimate form, rather than to reproduce the series of changes by which it may have arisen. An Oxytricha or a Vorticella, for instance, which may have been produced by the transformation of a Euglena or other algoid vesicle, multiplies its own form by fission or by the process of gemmation; and should the Vorticella encyst itself,

[1] See p. 88.

and become metamorphosed into a Rotifer, this in turn, when it multiplies, whether agamically or by true eggs, produces other Rotifers. Again, it has been ascertained that both Rotifers and Nematoids are capable of arising directly from transforming vegetal vesicles if they are of sufficient size, and provided they undergo some unknown though probably distinct processes of total molecular change. And the forms which have been thus produced also multiply their own kind, without exhibiting the least tendency to reproduce a vegetal organism similar to that from which the transforming vesicles had been derived.

As soon as new forms arise which habitually produce internal buds or eggs, such organisms may be separated in an important manner from those ephemeromorphs from which they have been produced. Amongst animals and plants in which such processes occur, we begin to find those definitely recurring forms constituting 'species,' to which we have above alluded, because homogenesis has now become the rule, and the sexual method of reproduction has more or less definitely commenced. This sexual differentiation, as we have seen, has been rapidly attained by some of the highest representatives of heterogenetic transformations, and some of these forms, such as Nematoids, afterwards continue to multiply themselves through successive generations by a sexual and homogenetic method.

Seeing that the product of transformation in each of the above-mentioned cases subsequently multiplies (as long

as it lasts) after the homogenetic method, the process itself must, in each of such cases, have been one of Heterogenesis. These transforming molecular changes must have been total and complete,—since all organic memory of the previous phases of such forms seems to have been effectually blotted out. This, however, appears to be by no means the case with all equally simple organisms. In some, the ascending grades of evolution by which the more perfect sexual type has been achieved, have probably been much more gradual, and where this is the case some of these grades may be preserved in the developmental changes which the fecundated germ subsequently goes through. The germ does not tend to develop immediately into a form similar to that from which it had been derived. It rather tends to grow at once into some antecedent form, and to attain to the likeness of its parent, by passing through developmental changes or metamorphoses, more or less similar to certain current heterogenetic changes which had been previously apt to occur.

Whenever, therefore, a given series of developmental changes or imperfect transformations has terminated in the production of a sexual form, and when these changes have not involved a period of total molecular rearrangement, the germ, by a power of 'reversion' (which is not unfrequent even among the higher forms of life) *may* tend to reproduce such changes rather than undergo an immediate development into

the parent form. And where development takes place by such successive stages—which, as it were, reproduce or copy previous current heterogenetic changes—the same tendency to multiply by an asexual process is manifested by the several forms which represent the different developmental stages, as was previously displayed by the several heterogenetic products.

Heterogenesis, therefore, would thus appear to be the essential underlying cause of all developmental transformations or metamorphoses; and, moreover, the hitherto unexplained phenomena of 'alternate generation' may, perhaps, be deemed to receive a rational and approximate interpretation [1].

[1] There would thus be three distinct modes by which sexual reproduction may make its appearance amongst the 'Ephemeromorphs':—

(*a*) It may occur as it were 'accidentally,' at long and altogether irregular intervals, during some of the later stages in a series of ephemeromorphic developments—and then in a very rudimentary manner. The best instances of this are, perhaps, to be found amongst Fungi. Thus I would suggest that a relationship of this kind probably obtains between Uredo, Æcidium, and Uromyces as a possible final form in which rudimentary sexual organs may become differentiated.

(*b*) It may occur, similarly, in one of the later stages of a series of ephemeromorphic transformations, though such changes may subsequently tend to recur so as to produce a case of cyclical or 'indirect' homogenesis. Portions of ephemeromorphic life are, as it were, thus nipped off and preserved, so as to constitute the different kinds of 'alternate generation' which are known to prevail both amongst animal and vegetal forms of life.

(*c*) It also occurs when an ephemeromorphic series ends abruptly—i.e. when by some final process of heterogenesis an organism is at once produced which subsequently develops sexual characters, and thenceforth multiplies by the homogenetic method. In this case the

These processes of 'alternate generation' formerly attracted very much attention, and were first prominently brought under the notice of naturalists by Steenstrup, in 1842[1]. Whether occurring amongst animal or vegetal forms, the processes are essentially characterized by the fact that a fertilized germ goes through two or more metamorphic stages before attaining the perfect form in which similar fertilized germs are evolved; whilst in each of the lower stages the immature forms possess the power of multiplying agamically. By common consent such a succession of forms is admitted to correspond to what is known as an organic 'species.' The fact of the metamorphosis could of course have no effect in negativing such a view, seeing that it has hitherto afforded no obstacle in the case of insects: although in instances of 'alternate generation,' a further complication, it is true, is introduced by the fact of the multiplication which takes place amongst the lower metamorphic forms, and by the fact that the successive stages are occasionally mere derivative forms produced by a process of 'gemmation.' The fact of the multiplication of the transition forms, however, cannot introduce any real difficulty, since a

heterogenetic germ is large, the organism develops without metamorphosis, and subsequently produces large germs or ova, which also go through a similarly 'direct' process of development. Instances of this occur amongst Nematoids and Rotifers, and probably also amongst some Medusæ, Trematodes, and other low forms of life.

[1] In his 'Generations-Wechsel,' of which a translation was published by the Ray Society in 1845.

process of fission occurs occasionally even in the very early embryonic stages of higher animals—in which the mass that undergoes the process, save for all its inherited potentialities, differs little from a mass of simplest protoplasm. And again, as we descend in the scale of organization, the lower forms of life are found to retain more and more of those tendencies commonly exhibited by the lowest forms of which they are the more or less remote descendants. Thus fission and gemmation exclusively prevail amongst the ephemeromorphs, and in what is called 'alternate generation' we also meet with such processes occurring most abundantly in association with recurring processes of metamorphosis, of which heterogenesis supplies the originals.

The transition, therefore, from the fissiparous multiplication of the germ mass which may take place in the early embryonic stage of man and other mammals, to the multiplication which takes place by a process of internal gemmation in the Aphides, and to those other processes which occur amongst the lower Invertebrata and Cryptogams—constituting the cases of so-called 'alternate generation'—is most gradual and legitimate. This may be seen from the following table, in which some of the principal types of 'alternate generation' have been stated in the simplest manner with the view of facilitating comparison, and thus leading to more comprehensive notions concerning the real nature of the changes which take place and their alliance to other more or less familiar processes:—

MODES OF DEVELOPMENT

IN RELATION TO

ASEXUAL MULTIPLICATION OCCURRING DURING ITS PROGRESS.*

Explanation of signs.
- (*a*) Signifies internal gemmation.
- (*b*) Signifies fission or external gemmation.
- (*c*) Budding, without separation.
- (*d*) Budding without separation at first, though subsequently separation of *some* or *all* of segments.
- (*e*) Reproduction by fertilized germs.

A. *Multiple derivative forms*—'alternate generation.'

1. *Distoma* germ. (*a*)
   1st Brood. (*a*)
   2nd Brood. (*a*)
   3rd Brood (Cercariæ).
   Encysted state.
   Distoma larva.
   Mature Distoma. (*e*)

2. Moss germ. (*b*)
   Protonema. (*d*)
   Protonema. (*c*)
   Incipient Moss. (*d*)
   Mature Moss. (*e*)

3. *T. echinococcus* germ.
   Active Embryo.
   Hydated Cyst. (*cd*)
   Echinococci. (*c*)
   Mature Entozoon. (*b*)
   Proglottides. (*e*)

4. Fern germ. (*c*)
   Immature Fern. (*c*)
   Mature Fern. (*b*)
   Spores. (*c*)
   Prothallium. (*e*)

5. *Myrianida* germ. (*c*)
   Larva. (*cd*)
   Mature Myrianidæ. (*e*)

6. *T. serrata* germ.
   Active Embryo.
   Cysticercus. (*cc*)
   Mature Entozoon. (*b*)
   Proglottides. (*e*)

7. *Aurelia* germ.
   Ciliated Embryo.
   Polype. (*cd*)
   Medusæ. (*e*)

8. *Sertularia* germ.
   Ciliated Embryo. (*c*)
   Polypidom. (*d*)
   Capsules. (*a*)
   Medusæ. (*e*)

9. *Coryne* germ.
   Ciliated Embryo. (*c*)
   Polypidom. (*d*)
   Polypidom. (*d*)
   Medusæ. (*e*)

10. *Salpa* germ.
    Larva Stock. (*da*)
    Mature Salpæ. (*e*)
    (aggregated)

11. Flowering Plant germ. (*cd*)
    Immature Plant. (*c*)
    Mature Plant. (*e*)
    (aggregated)

12. *Aphis* germ.
    Immature Aphis. (*a*)
    Immature Aphis. (*a*)
    &c.
    Mature Aphis. (*e*)

B. *Process intermediate between 'budding' and metamorphosis—product of Larva single.*

13. *Asterias* germ.
    Bipinnaria. (*a*)
    Immature Asterias.
    Mature Asterias. (*e*)

14. *Echinus* germ.
    Pluteus. (*a*)
    Immature Echinus.
    Mature Echinus. (*e*)

C. *Complete development of germ stock—with multiplication and metamorphosis.*

15. *Botryllus* germ (*b*)
    Immature Botrylli. (*c*)
    Mature Botrylli. (*e*)
    (aggregated)

16. *Ascidian* germ.
    Tadpole-like larva.
    Fixed Ascidian. (*c*)
    Mature Ascidians. (*e*)
    (aggregated)

17. *Bryozoa* germ.
    Ciliated Embryo.
    Fixed Bryozoan. (*c*)
    Mature Bryozoa.
    (aggregated)

D. *Complete development of germ—with or without multiplication, but with no metamorphosis.*

18. *Hydra* germ.
    Immature Hydræ. (*d*)
    Mature Hydra. (*e*)

19. *Eolis* germ. (*b*)
    Immature Eolis.
    Mature Eolis. (*e*)

20. Vertebrate germ. (*b*)
    Asexual Vertebrate.
    Mature Vertebrate. (*e*)

21. Vertebrate germ.
    Asexual Vertebrate.
    Mature Vertebrate. (*e*)

* Some of the most typical modes have been selected and stated in the simplest manner, with the view of facilitating comparisons and appreciation of real relationships.

From what has been hitherto said, it would appear that nothing comparable to those assemblages which have previously been understood to constitute a 'species' can be considered to exist till sexual generation begins to manifest itself and tends to recur. Such forms, however, have been, and are being, gradually evolved in ascending series from an enormous plexus of ephemeromorphs, which is itself being constantly reinforced from the inorganic world. This plexus is composed of untold legions of organisms—partly animal and partly vegetal, and of an inexpressibly varied size, shape and hue—many of which are possessed of an enormous power of independent self-multiplication, and of adapting themselves to changed conditions by taking on entirely new forms. We have now seen, moreover, that many of the earliest 'species' which appear are represented by a definite succession of varied forms—such as we meet with in the cases of so-called 'alternate generation.'

So that (referring to the question which we have already in part discussed[1]), as distinct 'species' are no longer found, in all cases, to consist of only one set of distinct individuals, and as no such assemblages of similarly recurring forms begin to exist till we arrive at plants and animals which have attained some degree of complexity, it is our notion of the meaning of this word 'species' which ought to undergo change, rather than our notion of the meaning of the word 'individual.' Individuality in every sense of the word must

[1] See pp. 553-557.

be conceded to the representatives of those vast multitudes of infusorial and cryptogamic forms which collectively constitute an ever-changing vital plexus, from the various portions of which well-marked animals and plants are constantly arising. Biological individuals, in fact, exist which multiply prodigiously by agamic processes long before a sexual mode of reproduction begins to manifest itself, and therefore long before 'true generative acts' are performed, and before 'species' begin to exist.

So that instead of considering the total product of any one fecundated germ as a single individual (even when it comprises what others would call hundreds of individuals), an alteration in our conception of the word 'species'—which is now actually necessitated—suffices to clear away all the old difficulties. We may quite easily recognize that, just as individuals are either simple, or complex and aggregated, so 'species' may be represented either by definitely recurring series of different individuals, derived one from another by agamic processes—though the last of the series develops rudimentary sexual organs in which fertilized germs are produced, constituting the first terms of the new series; or else by single (hermaphrodite) or by double (male and female) recurring individuals, each of the representatives of which undergoes a more or less metamorphic process of development.

The views which we have hitherto announced also

derive additional support from the fact that even amongst those groups of animals or plants in which 'alternate generation' largely prevails, some species undergo a direct process of development. No sort of explanation of these apparent anomalies has, as yet, been offered. But now it seems that they are rendered in some degree explicable by what we have learned concerning the transformations of some of the ephemeromorphs. We may find, for instance, a Rotifer originating directly from the metamorphosis of a large Euglena, or of a large mass of protoplasm and chlorophyll which separates from the wall of a Nitella-filament; whilst if a small Euglena, or a small mass of Nitella protoplasm and chlorophyll undergoes transformation, a more or less similar Rotifer may ultimately be developed, though in this case only after the organism directly produced from the originally smaller matrix had passed through certain metamorphic changes. Such a product of transformation may appear successively as an Actinophrys, an Amœba, and a Ciliated Infusiorum (and during each of these stages agamic multiplication may take place) before passing, as a much larger mass, into that final state of encystment from which it is to emerge in the form of a Rotifer. Such double sets of phenomena are very prone to occur amongst the ephemeromorphs, and are remarkably analogous to the direct development of some few eggs of Distomata or Medusæ as compared with the cyclical development of others by processes of 'alternate generation.'

The analogy is, however, rendered all the more striking, when we find that the forms of Medusæ and Distomata which undergo the exceptional direct process of development are all of them notable for the very large size of their eggs, whilst all those which develop indirectly, through intermediate forms, invariably start from much smaller eggs. It is difficult, therefore, to avoid the conclusion that these previously unexplained differences in the modes of origin of Medusæ and other organisms amongst which 'alternate generation' prevails, are probably referrible to some such differences as are now seen to obtain amongst Rotifers and other forms which are producible by a direct or an indirect origin from a transforming vegetal vesicle according as it happens to be large or small.

Variations and Transmutations of Species.

Facts of the kind to which we have just been referring, as well as multitudes of others, seem to show in the clearest manner possible, how strictly the Ephemeromorphs are comparable with Crystals. Their forms and structures seem to be as plainly the result of the organic polarities of the molecules entering into their composition, as the forms of crystals are due to the polarities of their constituent molecules. There is an infinitely greater diversity amongst the ephemeromorphs, it is true, on account of the extreme complexity of the different kinds of living matter, and of

their readiness to fall into new modes of combination —many of which necessitate new modes of moving equilibrium, which can only be brought about by the production of new organic forms.

The facts already cited suffice, therefore, to show that Natural Selection can have no sort of influence, as a producer of change, over the members of that vast assemblage of Infusorial and Cryptogamic organisms of which the ephemeromorphic world is composed—although Mr. Darwin appears to believe that the form and structure of 'every living thing' has been influenced by such a set of agencies [1]. For independently of the fact that laws of 'polarity' alone seem to prevail in determining the form and structure of the ephemeromorphs, these comparatively simple animal and vegetal forms are derived one out of another in a manner which is so irregular and variable at different times, that no 'laws of heredity' can come into play. And this being the case, Natural Selection cannot operate as a producer of variation. For, in briefly epitomizing his doctrine of Natural Selection, Mr. Darwin thus expresses it [2]:—'As many more individuals of each species are born than

[1] Thus Mr. Darwin says:—'*Every detail of structure in every living creature* (making some little allowance for the direct action of physical conditions) may be viewed either as having been of special use to some ancestral form, or as being now of special use to some of the descendants of this form—either directly or indirectly through the complex laws of growth.' See also quotations on p. 590, and p. 607, note.

[2] 'Origin of Species,' 5th ed., Introduction.

can possibly survive, and as, consequently, there is a frequently recurring struggle for existence, it follows that any being, if it vary, however slightly, in any manner profitable to itself, under the complex and sometimes varying conditions of life, will have a better chance of surviving, and thus be *naturally selected.* From the strong principle of inheritance, any selected variety will tend to propagate its new and modified form. . . . Thus the small differences distinguishing varieties of the same species steadily tend to increase, till they come to equal the greater differences between species of the same genus, or even of distinct genera.'

Obviously no such cause of modification can come into play until homogenesis becomes the rule, and the continued influence of heredity is rendered possible. So that Natural Selection can have nothing to do with the modifications of that vast assemblage of animal and vegetal forms which we have proposed to include under the name of 'ephemeromorphs.' It must, indeed, be limited to 'species,' such as we now define them; and we propose very briefly to consider to what extent such agencies operate amongst these forms as producers of variation, and to what extent they are supplemented by the action of other modifying influences.

It was pointed out by Mr. Herbert Spencer[1] in 1864 that a certain amount of ambiguity attached to

[1] 'Principles of Biology,' vol. i. p. 445.

the phrase 'Natural Selection,' although, so far as I know, there was no indication that the two meanings of the phrase had ever been thoroughly distinguished by Mr. Darwin. On this subject Mr. Spencer wrote as follows:—'That organisms which live thereby prove themselves fit to live, in so far as they have been tried; while organisms which die, thereby prove themselves in some respects unfitted for living; are facts no less manifest than is the fact that this self-acting purification of a species must tend ever to insure adaptation between it and its environment. This adaptation may be either so maintained or so produced. . . . To recognize Natural Selection as a means of preserving an already established balance between the powers of a species and the forces to which it is subject, is to recognize it only in its simplest and most general mode of action. It is the more special mode of action with which we are here concerned. This more special mode of action Mr. Darwin has been the first to perceive[1]. To him we owe the discovery that Natural Selection is capable of *producing* fitness between organisms and their circumstances; and he, too, has the merit of appreciating the immensely important consequences that follow from this. He has worked up an enormous

[1] It is now very generally known that the discovery was independently made and almost simultaneously published by Mr. A. W. Wallace—although Mr. Wallace does not claim to have elaborated the theory with anything like the thoroughness or breadth of illustration which has characterized Mr. Darwin's work. (See Wallace's 'Contribution to the Theory of Natural Selection,' 2nd ed. 1872.)

amount of evidence into an elaborate demonstration, that this "preservation of favoured races in the struggle for life" is an ever-acting cause of divergence amongst organic forms. He has traced out the involved results of the process with marvellous subtlety. He has shown how hosts of otherwise inexplicable facts are fully accounted for by it. In brief, he has proved that the cause he alleges is a true cause.'

But the general process, which is universal in its operation, must be distinguished from the more special process, which is less extensive in its range of application. And accordingly Mr. Spencer, whilst clearly recognizing the immense importance of Natural Selection (or 'survival of the fittest,' as he aptly renders it) as a cause of specific transmutation, nevertheless recognizes it as only one of the causes which in the course of time suffices to produce new species out of pre-existing species. Thus any altered form or structure directly brought about by a change in the external conditions to which certain organisms are exposed may be transmitted and perpetuated through the same principle of inheritance; and so also may functionally-produced changes in organisms be perpetuated in a similar manner. The two latter processes come under the head of what Mr. Spencer terms '*direct equilibration*,' whilst Natural Selection, so far as it is a 'producer' of change, acts by a process of '*indirect equilibration*.'

Mr. Darwin, on the contrary, appears not to recognize the distinctions above indicated, since he professes to

look upon all causes of variability as being mere starting-points for the action of Natural Selection[1]. This is quite obvious from his general treatment of the subject, and such a view is also very distinctly enunciated in the following passage, where he says[2]:—'We know, however, far too little of the causes and laws of variation to make a sound classification. The direct actions of the conditions of life, whether leading to definite or indefinite results, is a totally distinct consideration from the effects of Natural Selection; for *Natural Selection depends on the survival under various and complex circumstances of the best fitted individuals, but has no relation whatever to the primary cause of any modification of structure.*' The passage we have italicized seems to show most unmistakeably that the distinction between Natural Selection as a *maintainer* of already established forms, and Natural Selection as a *producer* of new forms, is not recognized by Mr. Darwin; or, on the other hand, if recognized, it must have been deliberately rejected. Mr. Darwin, in fact, attaches so great an importance to Natural Selection as a producer of change, and so little to the influence of the more

[1] Which, of course, is perfectly true, if we simply use the phrase in its more general sense, although it is somewhat contradictory in relation to the more special and more essentially Darwinian meaning of the term. The fact that these two meanings of the term are not clearly distinguished from one another by Mr. Darwin, has, I think, led to certain misunderstandings on the part of some writers respecting the real nature of his views.

[2] 'Animals and Plants, &c.' vol. ii. p. 272, 1868.

direct agencies recognized by others, that he almost neglects them as independent producers of variation and gives to the special agency a general range and applicability[1].

Influence of Change in External Conditions.

If we take the three modes already referred to, or any others, by which variations in the form or structure of animals and plants are initiated, we find that in each alike, if the change is to be a permanent one, the principle of inheritance must come into play. It is therefore only amongst the 'primary causes' of variation that any room for difference exists. Thus we may have:—(1) some altered incidence of external forces directly bringing about an altered internal action in the complex moving equilibrium which the organism represents, and the perpetuation of such alteration by inheritance, under the persistence of similar conditions; (2) the gradual induction of some altered structure by modifications initiated in the functional activity (indirect influence of conditions) of some part of an organism, which alteration is similarly perpetuated; and (3) the perpetuation and intensification by inheritance, through several generations, of some one or more out of the many individual differences which are always manifesting themselves in the different

[1] Although of late Mr. Darwin has appeared more disposed to limit the supposed influence of Natural Selection. (See note 3 of next page.)

representatives of a species—the 'natural selection' of some particular difference in these cases being determined by the advantage which fitness gives in the 'struggle for existence.'

It is of course quite true that the first two causes of variation, like the more minute and fortuitous instances of variability, may be made use of as starting-points for the further action of Natural Selection where the struggle for existence is severe; and so far Mr. Darwin's view would be thoroughly in accordance with the facts. But it seems equally clear from the vast amount of evidence, general and particular, which Mr. Spencer has adduced[1], that the direct and indirect influence of the conditions of life are really independent producers of specific variability amongst most of the forms of life outside the ephemeromorphic world[2]. So that they must stand side by side with Natural Selection, if not as co-equals, yet as occupying marked and important positions[3]. The evidence cited by Mr. Spencer concerning the direct influence of external conditions is particularly strong in many of

[1] See 'Principles of Biology,' vol. i. p. 184, and vol. ii.

[2] Many of which are 'adaptive changes.'

[3] Mr. Darwin has lately, whilst admitting the possibility of his having attached rather too exclusive an importance to the influence of Natural Selection, asserted that his main objects in the 'Origin of Species' were 'firstly, to show that species had not been separately created; and, secondly, that Natural Selection had been the chief agent of changes, though largely aided by the inherited effects of habit, and slightly by the direct action of the surrounding conditions.' ('Descent of Man,' chap. iv. 1870.)

its bearings upon the shapes and modes of growth of plants of different kinds, and is applicable even to their most minute details of structure. Amongst animals, however, the indirect influence of change in external conditions (that which acts by inducing modified function, or 'use and disuse') is also frequently recognizable[1]—this mode of operation being favoured by the fact that the functions of animals are more differentiated than those of plants. As Mr. Spencer says, in order that 'a new external action may be met by a new internal action, it is needful that it shall either continuously or frequently be borne by the individuals of the species, without killing or injuring them[2].' If the new influence act immediately upon the nutrition of the organism in a way which we are unable to explain (as is so frequently the case amongst plants), we regard it as a case of the direct influence of change in conditions; whilst if the change acts (as is frequently, though by no means invariably, the case amongst animals) in more slowly bringing about some obvious difference in function, then the difference of function is generally credited with bringing about the variation, and the action of the change in conditions is at least said to be 'indirect.' Changes brought about by 'use and disuse,' in reality, therefore, belong to the same category as those other changes which are said to be

[1] 'Principles of Biology,' vol. i. p. 439.
[2] Loc. cit., vol. i. p. 442.

produced by the influence of altered conditions. And it is often very difficult to make any distinction between the two modes of change, owing to the almost imperceptible gradations by which they tend to merge into one another.

What we have last said may be illustrated by quoting a few of the examples cited by Mr. Darwin, in which more or less marked changes in organisms have been distinctly determined by some alteration of the 'conditions' to which they had been previously exposed. Mr. Darwin says[1]:—'With respect to the common oyster, Mr. F. Buckland informs me that he can generally distinguish the shells from different districts; young oysters brought from Wales, and laid down in beds where "natives" are indigenous, in the short space of two months begin to assume the "native" character. M. Costa has recorded a much more remarkable case of the same nature, namely, that young shells taken from the shores of England and placed in the Mediterranean, at once altered their manner of growth, and formed prominent diverging rays, like those on the shells of the proper Mediterranean oyster. The same individual shell, showing both forms of growth, was exhibited before a Society in Paris.' Other direct effects of change in external conditions are perhaps brought about more slowly; thus, amongst many other examples, Mr. Darwin

[1] 'Animals and Plants under Domestication,' vol. ii. p. 280.

cites the following:—'The wood of the American locust-tree (*Robinia*), when grown in England, is nearly worthless, as is that of the oak-tree when grown at the Cape of Good Hope. Hemp and flax, as I hear from Dr. Falconer, flourish and yield plenty of seed on the plains of India, but their fibres are brittle and useless. Hemp, on the other hand, fails to produce in England that resinous matter which is so largely used in India as an intoxicating drug. . . . The fruit of the melon is greatly influenced by slight differences in culture and climate. . . . It is well known that American varieties of the apple produce in their native land magnificent and brightly-coloured fruit, but in England of a poor quality and a dull colour.' Again, twenty-nine kinds of American trees, belonging to different orders, have been compared with their nearest European allies by Mr. Meehan[1], and have almost invariably been found to differ from the latter in many similar respects, *e. g.* in the leaves being less deeply serrated, whilst they fall earlier and have a brighter tint, in the buds and seeds being smaller, and in the trees being more diffuse in growth. Mr. Darwin agrees with Mr. Meehan in thinking that these differences must almost exclusively have been 'caused by the long-continued action of the different climate of the two continents on the trees.' Different climates also affect in a most marked manner, in the course of two or three generations,

[1] 'Proc. Acad. Nat. Soc. of Philadelphia,' Jan. 28, 1862.

the hairy coverings of sheep, goats, dogs, cats, horses, and other animals [1].

Many of such instances cited by Mr. Darwin and other writers, abundantly testify to the fact that changes are frequently induced, both amongst animals and plants, by alterations in the conditions of life. And these changes are of course liable to become intensified in the course of time. On the other hand, we know quite well that other animals and plants, which happen to have a less impressionable constitution, may range over large portions of the world without undergoing any appreciable modification [2].

Internal Causes of Change and 'Progressive Development.'

The divergence of opinion to which we have already referred is, however, as nothing compared to that which obtains concerning the existence or non-existence of certain 'principles' or tendencies inherent in the organism itself, and leading, in the main, to an increasing complexity of organization.

The existence of such 'principles' or tendencies has been frequently affirmed, though even more frequently denied, either implicitly or explicitly.

[1] 'Animals and Plants under Domestication,' vol. ii. p. 278.

[2] Similarly we find that the majority of crystals may be exposed to the incidence of a considerable change in conditions, without producing or necessitating molecular rearrangements, though others, such as mercuric iodide, are most sensitive to changing influences, under which they are apt to assume not only changes of form but also complete alterations of colour. (See p. 82.)

Amongst those who do believe in the existence of internal causes of organization, quite different opinions are expressed by different writers as to their immediate origin or nature. Some, amongst whom we may name Dr. Erasmus Darwin[1] and the author of the 'Vestiges of Creation,' believed that such a principle had been specially implanted by a Great First Cause in a few first created forms, which, by virtue of this power, have sufficed during successive generations, and in the long lapse of ages, to engender all the forms of life that have ever appeared upon the surface of the earth. Prof. Owen[2] and Mr. St. George Mivart[3] have expressed the belief that a continuous origination of living matter has been ever taking place, and that this has been undergoing development in obedience to internal tendencies, which work, however, in certain preordained modes, with the view (amongst many others) of preparing for the ultimate appearance of Man[4]. But whilst Prof. Owen seems to think that the working out of these inherent tendencies may

[1] 'Zoonomia,' vol. i. pp. 500-510. 1794.

[2] 'Anatomy of the Vertebrates,' vol. iii. 1867.

[3] 'Genesis of Species,' 2nd ed., 1872.

[4] This innate capacity or power of undergoing change, whatever forms it may tend to evolve, is recognized by Prof. Owen as being part of a 'defined and pre-ordained course.' And he says (loc. cit., p. 808):—'A purposive route of development and change, of correlation and interdependence, manifesting intelligent Will, is as determinable in the succession of races as in the development and organization of the individual. Generations do not vary accidentally in any and every direction, but in pre-ordained, definite, and correlated courses.'

be adequate to account for the whole nature of man, Mr. Mivart lays less stress upon the miraculously-endowed origin of the internal tendencies, though he considers that some supernatural interposition must have been needed in order to produce man's moral faculties.

Lamarck [1], Professor Grant [2], and others, however, whilst holding that living matter was and is constantly being evolved by the operation of those laws which reign supreme in the inorganic world, also believe that organization increases in complexity, in obedience to naturally implanted internal tendencies—these internal tendencies being regarded as further consequences dependent upon, and harmonious with, those all-pervading natural 'laws' which continually lead to the formation of the living matter. Concerning the cause of the 'laws' themselves, they, in the absence of all evidence, do not feel called upon to express an opinion [3].

[1] In the *Introduction* to his 'Hist. Nat. des Animaux sans Vertébrés,' 1815.

[2] See p. 165, and also his 'Outline of an Elementary Course of Recent Zoology,' 1861, pp. 1–9 and 91.

[3] It is to be regretted that on other subjects Lamarck did not exhibit the same amount of caution. It is now quite commonly known that he endeavoured to support his 'progressionist' views by advocating the efficacy of modifying causes, which were most crudely conceived and, in fact, non-existent. Both he and Dr. Erasmus Darwin supplement the action of their respective 'internal tendencies' by the supposed action of mysteriously generated desires, appetites, and needs—and they imagined, moreover, that by the action of these postulated internal affections,

On the other hand, amongst the most prominent of those writers who utterly deny the existence of any internal tendency leading to increased complexity of organization, we are compelled to mention the names of Mr. Herbert Spencer and Mr. Darwin.

Mr. Spencer's position in this respect is, as we have already endeavoured to show, somewhat anomalous, inasmuch as it seems to amount to a practical denial of the 'instability of the homogeneous,' which in other places is affirmed to be inevitable and to be one of the 'First Principles' of that Evolution philosophy which he has so admirably expounded. Homogeneous matter generally is prone to undergo internal rearrangements, partly under the influence of incident forces and partly by reason of internal forces or polarities. Homogeneous living matter is of all forms of homogeneous matter that which is most favourably constituted for undergoing such internal rearrangements. Its molecular mobility is extreme, and it must always be exposed to varying external influences tending to produce change—although the effective action of these forces is always intimately dependent upon the conjoint activity of

changes could be produced which such volitional agencies were wholly incapable of bringing about. (See Herbert Spencer's 'Principles of Biology,' vol. i. p. 405.) Their notions as to the way in which 'adaptation' was produced were therefore quite crude and unreal. Both these writers, however, also appeal to the modifying influence which may be wrought by changed external conditions.

internal polarities. The constant co-operation of these causes of changes must, however, tend to bring about an increasing complexity of structure—although such increase may be checked and limited in various ways, and by causes which we, as yet, very imperfectly understand. Granting, however, that 'living' matter like all other forms of matter does tend to become more heterogeneous—what is it which, in the main, is the cause of this tendency? It would not be deemed incorrect if we were to state that the form of any particular crystal is determined by the polarities of its molecules, although such polarity and consequently such form may be modified by a change in the 'conditions' under which crystallization takes place; and similarly, it would seem fair to say (as, in fact, Mr. Spencer does elsewhere constantly affirm) that the form of an organism is determined by the polarities of its molecules. New incident external forces can, in fact, only induce changes so long as the elements of the living matter upon which they act are capable of being diverted by such forces into new modes of motion—that is, only so long as the molecular composition of the matter is capable of being altered by them, however slightly. But new modes of motion imply new modes of growth and development, which again could not be initiated unless internal polarities continued to exist as ever-active causes of change. It is, therefore, in every way, as it seems to me, a contradiction of Mr. Spencer's other arguments when he both denies that organisms possess

any internal tendency to vary, and explicitly states[1] that the environing forces are 'the source of the *power* which effects the rearrangement' under the influence of changed conditions, and that the polarities of the molecules simply determine 'the *direction* in which that power is turned.' Starting from his own 'First Principles' it would seem that lower organisms must possess an internal tendency to vary; whilst to attribute so much influence to external rather than to internal causes of change is about as convincing as if Mr. Spencer were to assert that the heat which ignites one extremity of a long train of gunpowder is the cause of the effects produced, or 'the source of the power' unlocked, by the explosion along the whole line[2].

Mr. Spencer has been led to adopt these doctrines, apparently, in order to account for the fact of the existence of multitudes of undifferentiated organisms in the present day, and with the view of explaining the apparent persistence of multitudes of other low types of life through long geologic ages. As he did not believe, when his 'Principles of Biology' was written, that any new evolution of living matter had taken place within recent ages, multitudes of known facts must have seemed quite irreconcilable with Mr. Spencer's general

[1] Appendix to 'Principles of Biology,' vol. ii. p. 488.

[2] Further arguments in support of the importance of internal tendencies in producing an increasing complexity of organization are to be found at pp. 123–129.

doctrines of Evolution. The arguments in favour of these general doctrines were, however, quite overwhelming, and could not be shaken. The difficulties, therefore, had to be explained in some manner; but unfortunately instead of facing the adverse criticisms of biologists and following out his own doctrines to their logical conclusions, Mr. Herbert Spencer attempted to reconcile the apparent discrepancies by denying that there existed in organisms any internal tendency to progressive differentiation. It is true, there were only two alternatives; and not being satisfied as to the present occurrence of the evolution of living matter, he had no choice but to accept the other supposition, and with it all the contradictions which it involved.

So long, however, as there occurs a changing incidence of external forces, Mr. Spencer is quite willing to concede that an increased complexity of organization must result; because, as he expresses it, 'a liability to be unfolded arises from the actions and reactions between organisms and their fluctuating environments[1].' And how all-pervading such changes are may be imagined when Mr. Spencer habitually formulates them as being produced by 'astronomic, geologic, meteorologic, and organic agencies.' That the long-continued action of such agencies tends to produce cumulative effects and progressively higher organization in some of the forms, is explicitly taught by Mr. Spencer, since he speaks of

[1] 'Principles of Biology,' vol. i. p. 431.

'the average result' being, 'that on previous complications of structure wrought by previous incident forces new complications are continually superposed by new incident forces. And hence simultaneously arises increasing heterogeneity in the structures of individuals, in the structures of species, and in the structures of the earth's Flora and Fauna[1].' But with this constant alternation of changes of all kinds, to which every single environment on the face of the earth must have been subjected, is it to be deemed possible that the descendants of simple and little differentiated organisms could have reproduced their like, without any considerable alteration, during an unbroken lineal descent through the millions and millions of years which must have elapsed since the first evolution of life upon our planet? Mr. Spencer seems to think this is possible, owing to the fact that occasionally 'new influences are escaped by the survival of species in the unchanged parts of their habitats, or by their spread into neighbouring habitats which the change has rendered like their original habitats, or by both.' But, independently of the impossibility of eluding the all-pervading influence of some of the causes of change above cited, it seems to me absolutely incredible that, through this long lapse of ages, some of the lineal descendants of so many specific forms should have succeeded in

[1] This was also, in the main, the view advocated by Geoffroy St. Hilaire in his 'Etudes progressives d'un Naturaliste,' p. 107. See also 'Mém. de l'Acad. des Sciences,' 1833.

'dodging' all changes in their environment, after a given form had been attained.

Mr. Darwin, however, expressly states that he is not an advocate of 'progressive development.' He says[1]:—'On our theory the continued existence of lowly organisms offers no difficulty; for Natural Selection, or the survival of the fittest, does not necessarily include progressive development—it only takes advantage of such variations as arise, and are beneficial to each creature under its complex relations of life. And it may be asked, What advantage, so far as we can see, would it be to an infusorian animalcule, to an intestinal worm, or even to an earth worm, to be highly organized? If it were no advantage, these forms would be left by Natural Selection unimproved or but little improved, and might remain for indefinite ages in their present little advanced condition.'

In certain other respects, however, Mr. Darwin's views are much more in accordance with the views of those who believe in the existence of an internal principle or tendency leading to progressive complexity of development. He thinks, for instance, in opposition to Mr. Spencer, that where change is brought about in any organism by the incidence of new conditions, the nature of the organism itself—that is, the peculiar qualities or modes of action which go on in it—are of much more importance in determining the result than the

[1] 'Origin of Species,' 5th ed., p. 145.

nature of the conditions themselves[1]. He comes to this conclusion because, as he says, 'instances could be given of similar varieties being produced from the same species under external conditions as different as can well be conceived; and, on the other hand, of dissimilar varieties being produced under apparently the same external conditions.'

Numerous facts and reasons might be cited in favour of the correctness of this view. It has long been familiar, for instance, to all who have studied the lowest forms of life, that the organisms which appear in infusions of different organic substances are often quite different in character; whilst each particular kind of decaying substance is characterized by its own peculiar form of Mould. Then, again, we know that alterations

[1] See 'Origin of Species,' 5th ed., pp. 8 and 166. Prof. Owen, moreover, is of the same opinion. The influence of internal tendencies, he says, is displayed by the fact that in a comparatively defined theatre 'the various polypes of the coral reef display their diversities of colour, size, shape, and structure, independently of outward influences.' He says that, 'of the 120 kinds of coral enumerated by Ehrenberg in the Red Sea, 100 at least exist under the same conditions.' ('Anatomy of the Vertebrates,' vol. iii. p. 808.) On the other hand, the polypes of a coral reef must remain essentially the same kinds of organisms (however subject to minor changes) during the many thousands of years in which the reef has been forming, and this fact is more in accordance with Mr. Spencer's notions than with my own, unless—as I am inclined to think—the inherent causes of change whose influence is at first manifest in producing differentiation, become more and more diminished in intensity after even a rudimentary organization has been attained. So that after a time that change of conditions, which Mr. Spencer believes to be so all-essential, may become necessary in order to induce further variation. (See p. 608.)

in the quality of an infusion, however induced—whether by the agency of heat or by the addition of a minute quantity of some chemical ingredient—will frequently cause entirely different organisms to appear in altered and unaltered portions which are similarly exposed[1]. And yet different portions of the same unaltered solution, when it is highly fermentable, are found at first to yield nothing but Bacteria, under the most varied conditions of exposure to air, or even *in vacuo*.

The above-mentioned variations in the organic products obtainable from different infusions, or from the same infusion after its organic constituents have been slightly modified, are, therefore, now best explicable on the supposition that from these different starting-points living matter comes into existence with initial, though probably inconsiderable, molecular differences of constitution, and that these variations lead the respective new-born units to assume different modes of growth[2].

Again, the fact that lower Infusoria and lower Cryptogams are to be met with, possessing almost similar forms and characters in the most different regions of the earth, seems to show pretty conclusively that the

[1] See p. 302.

[2] The different results, as regards form and structure, producible by difference in the composition of the aggregating material, have, moreover, been beautifully demonstrated by Mr. Rainey (pp. 60–65) in his experiments on the artificial production of calculi—which he has shown to be modified forms of crystals. The supposition above mentioned would harmonize well with an expansion of Mr. Spencer's hypothesis concerning 'psychological units.' (See p. 595, note 1.)

intrinsic properties of the more or less similar matter from which they have been derived, have also more to do with their forms and structures than any differences in the conditions under which they have been born. And yet we find that many of such lower organisms are quite unable to adjust themselves to slight changes in their 'conditions of life.' When these overtake them, they either die or become converted into new forms—lower or higher as the case may be[1]. And whether any particular change of conditions can or can not produce direct effects upon an organism, seems to depend principally upon the nature of the organism itself. This is undoubtedly the case with crystals; and since it appears also to hold good for organisms, it ought to impress us with a conviction of the immense importance of the aggregate polarities of the organism and of the exact nature of the complex moving equilibrium which exists in each case, in reference to the possible influence of any particular change in external conditions.

Although, therefore, Mr. Darwin does not believe

[1] Thus it would appear that an essential similarity exists amongst the forms encountered in different regions (Pritchard's 'Infusoria,' p. 375), because the elementary forms of vegetal matter (from which so many of the Infusoria and Cryptogams are derived) have an essentially similar nature in these various regions (see p. 613). The several forms unfold, therefore, more or less immediately into such and such organisms, according to the molecular composition of the matrices from which they start. But if new influences impinge upon such impressionable organisms, they, for the most part, either die, or else the matter of which they are composed undergoes some marked heterogenetic transformation, by which higher or lower organisms may be produced.

in the *continuous* operation of any internal tendency, whether miraculously or naturally impressed upon living matter, which may lead it to exhibit a tendency to become more and more heterogeneous—he, to a certain extent, verges in the direction of those who do believe in the existence of such a tendency. He admits the great importance of the actual constitution of the organism itself, and also the ease with which most important differences in organic form and structure occasionally spring up, by virtue of internal molecular modifications which seem to occur quite independently of any external causes of change.

And in spite of all that Mr. Darwin has said concerning his belief in the all-important influence of Natural Selection in bringing about specific modifications, we think that many of the very interesting facts which he has recorded in that treasury of biological knowledge, his 'Animals and Plants under Domestication,' afford sufficient evidence to prove that Mr. Darwin did not assign sufficient importance to other causes, as producers of specific variation[1]. We have already alluded to the comparatively low estimate which he attaches to the influence of change in external conditions[2]. We think, moreover, that Mr. Darwin

[1] In this respect, therefore, we agree with Mr. St. George Mivart. (See his 'Genesis of Species,' chap. iv.)

[2] The question whether changes are often initiated in this manner (see pp. 578-582) is, of course, quite independent of the one more recently discussed, which was as to the relative importance of the internal and of the external agencies in the consideration of changes so induced.

failed to attach an adequate importance to such instances of 'spontaneous' variation as he has recorded. These instances seem to afford very interesting examples of the operation in higher organisms of those influences which suffice to produce such multitudes of heterogenetic changes amongst lower organisms; so that they supply most valuable additional testimony as to the continued influence of 'organic polarity' in determining the form and structure of higher organisms[1]. This organic polarity is believed by Mr. Spencer and others to regulate the forms of organisms throughout all their developmental stages[2]; and, as we have already endeavoured to show, the wonderful powers of repair exhibited by crustacea, fish, amphibia, and

[1] Believing that living organisms are ultimately compounded of exceedingly complex molecules or 'physiological units,' exhibiting multitudes of minute differences amongst themselves, Mr. Spencer says such 'specific molecules, having the immense complexity above described, and having correspondently complex polarities which cannot be mutually balanced by any simple form of aggregation, have for the form of aggregation in which all their forces are equilibrated, the structure of the adult organism to which they belong, and they are compelled to fall into this structure by the co-operation of the environing forces acting on them and the forces they exercise on one another.' (Appendix to 'Principles of Biology,' vol. ii. p. 488.)

[2] Appendix to 'Principles of Biology,' vol. ii. p. 489. Prof. Grant also writes in the same strain when he says:—'No animal can be formed . . . . . . without its passing through the entire long series of developmental changes peculiar to each. Whether the product of the nucleus is to be a Monad or a Whale, *each successive stage of the process of formation has within itself the sole and exclusive potentiality of the next stage in advance*, and none therefore can ever be overstepped or omitted.' ('Recent Zoology,' 1861, p. 89.)

multitudes of other less complex organisms, seem only explicable in accordance with such a notion[1].

If looked at from these points of view, we shall be more fully able to appreciate the importance of many of the instances cited by Mr. Darwin. One of the simplest and yet most satisfactory examples is that recorded concerning the rare and occasional production of nectarines upon peach-trees, and the reverse. Speaking of the peach, Mr. Darwin says[2]:—'This tree has been cultivated by the million in various parts of the world, has been treated differently, grown on its own roots and grafted on a stock, planted as a standard, against a wall, and under glass; yet each bud of each sub-variety keeps true to its kind. But occasionally, at long intervals of time, a tree in England, or under the widely different climate of Virginia, produces a single bud, and this yields a branch which ever afterwards yields nectarines. Nectarines differ, as every one knows, from peaches in their smoothness, size, and flavour; and the difference is so great that some botanists have maintained that they are specifically distinct. So permanent are the

[1] In addition to the cases of repair and reproduction of lost parts already alluded to (p. 88), we are now in a position to refer to others occurring even in mammals. Thus it has been ascertained by M. Peyraux and M. Phillipeaux that the spleen is restored in animals after its extirpation, provided a minute portion of the organ is left *in situ* ('Ann. des Sci. Nat.,' Zool., Jan. 1867); and Dr. Carpenter ('Comparative Physiology,' 4th ed. p. 480) refers to interesting cases in which in the human subject the thumb and the ramus of the lower jaw were reproduced after removal.

[2] 'Animals and Plants under Domestication,' vol. i. pp. 337, 339-342.

characters thus suddenly acquired, that a nectarine produced by bud-variation has propagated itself by seed.'

Changes of this kind—and several others which Mr. Darwin records[1]—are doubtless due to some molecular modification, brought about in an unknown manner, in the tissues of the bud which varies; so that the production of the nectarine structure is the result of the altered balance and the new moving equilibrium which becomes necessitated in the tissues of the growing part. Such an explanation of these apparently 'spontaneous' changes in plants may be illustrated by alterations which are liable to occur in certain parts of animals when they are exposed to particular influences. Thus Mr. Wallace says[2]:—'The Indians (of South America) have a curious art by which they change the colours of the feathers of many birds. They pluck out those from the part which they wish to paint, and inoculate the fresh wound with the milky secretion from the skin of a small toad. The feathers grow of a brilliant yellow colour, and on being plucked out, it is said, grow again of the same colour without any fresh operation.' Although this change seems to be producible at will by a definite agent, we really know no more concerning the actual steps of the process by which it is produced, than we know concerning the intimate nature of the

[1] Somewhat analogous changes, for instance, occasionally occur in rose-trees, whereby a moss-rose suddenly appears upon a tree belonging to a totally different variety (loc. cit., pp. 379–381).

[2] 'Travels on the Amazon and Rio Negro,' p. 294.

changes by which the peach-branch is metamorphosed into a nectarine-branch. The possibility of the occurrence of the molecular change which occasions this metamorphosis is, moreover, by no means limited to the buds of the peach-tree. For, as Mr. Darwin tells us, 'nectarines have likewise been produced from the stone of the peach, and, reversely, peaches from the stone of the nectarine.' And even this is not all, since 'the same flower-bud has yielded a fruit one half or one quarter a nectarine, and the other half or three quarters a peach.'

Again, as a proof that at rare intervals, what appear to be totally distinct specific forms may arise from the embryo of a given species of animal, we may cite the most remarkable instance[1] of the appearance of the 'black-shouldered peacock' (*Pavo nigripennis*) amongst Sir J. Trevelyan's flock, composed entirely of the common species. The new form increased 'to the extinction of the previously existing breed,' and it is regarded by several of our best authorities as a distinct 'species': and yet this black-shouldered peacock has been known to have had a similarly independent origin five times, in England. There is no real reason, therefore, why such changes should not also at times occur with plants or animals living in the wild state. Mr. Murphy aptly remarks[2]:—'It may be true that

[1] Darwin's 'Animals and Plants under Domestication,' vol. i. p. 290.

[2] 'Habit and Intelligence,' vol. i. p. 344.

we have no evidence of the origin of wild species in this way. But this is not a case in which negative evidence proves anything. We have never witnessed the origin of a wild species by any process whatever; and if a species were to come suddenly into being in the wild state, as the Ancon sheep did under domestication, how could you ascertain the fact? If the first of a newly-begotten species were found, the fact of its discovery would tell nothing about its origin. Naturalists would register it as a very rare species, having been only once met with, but they would have no means of knowing whether it were the first or the last of its race.'

How is it possible, therefore, to gauge the amount of this kind of change which takes place amongst higher organisms, whereby new species may more or less abruptly appear upon the scene? Indeed, Mr. Darwin himself says:—'With such facts before us, we need feel no surprise at the appearance of any modification in any organic being.' Again, it must be almost certain that in such cases we have to do with changes in molecular constitution, similar in kind to those which amongst simpler organisms sufficed to produce many of the startling metamorphoses that have been recorded in this work.

We have endeavoured briefly to embody our own views concerning the causes which determine and

which subsequently modify the forms and structures of Organisms in the following table:—

Forms of	*Primarily determined by*	*Subsequently modified by*		*Nature of forms affected.*
CRYSTALS	Polarity.			
ORGANISMS (**Ephemeromorphs**).	Polarity*	1. External changes + *Polarity.*	*a.* Direct influence	**Ephemeromorphs and Species.**
			b. Indirect influence ('use and disuse')	**Species.**
		2. Internal changes + *Polarity.*	*a.* Leading to important but not permanent changes	**Ephemeromorphs.**
			b. Leading to important and permanent alterations which become transmitted	**Species.**
			c. Leading to individual differences, some of which are intensified and made permanent by *Natural Selection*	**Species.**
		3. Changes occasioned by 'crossing' + *Polarity*		**Species.**

* This word is used in the sense in which it is employed by Mr. Spencer, who says: 'If we accept the word *polarity* as a name for the force by which inorganic units are aggregated into a form peculiar to them, we may apply this name to the analogous force displayed by organic units. But as above admitted, polarity as ascribed to atoms is but a name for something of which we are ignorant . . . we simply substitute the term "polarity" for the circuitous expression, "the power which certain units have of arranging themselves into a special form."' ('Principles of Biology,' 1864, vol. i. p. 181.)

It should not be forgotten, moreover, that Natural Selection in its most general sense has reference to that co-ordinating power in Nature whereby the fittest of all organisms, of whatsoever kind and howsoever produced, tend to be perpetuated; whilst Natural Selection in its more special sense is an agency which Mr. Darwin has shown to be capable of 'producing fitness between organisms and their circumstances' by perpetuating and intensifying any minute variations of a favourable nature[1]. The latter is, however, only one amongst other agencies which are capable of giving rise to specific transmutations, although Mr. Darwin often speaks of

[1] See p. 573.

it as if it were as universal in its operation as the general co-ordinating power above referred to, which does more or less influence all forms of life. Again, instead of looking upon 'organic polarity' as an ever potent, co-ordinating power, which must infallibly co-operate in producing the results that happen to be initiated by any cause of change whatsoever, Mr. Darwin seems to regard it as a more or less occasional cause of changes otherwise unaccountable—of which he gives many highly interesting examples under the head of 'Correlated Variability[1].'

Having made out with admirable skill one of the principal modes by which, in the course of time, higher organisms are made to vary, Mr. Darwin seems unwilling to admit that other agencies may occasionally be much more potent—especially amongst lower organisms. And yet internal causes, or molecular polarities, are, as we have seen, almost the sole regulators of form and structure amongst those multitudinous hosts of lower organisms now included under the name of Ephemeromorphs[2]. These internal polarities, operating

[1] 'Animals and Plants under Domestication,' chap. xxv.

[2] As we have previously stated (p. 594), Mr. St. George Mivart is also strongly impressed with the inadequacy of Natural Selection for occupying the all-important position assigned to it by Mr. Darwin. He has expressed his belief that there is some deep underlying and internal cause of change to which the influence of Natural Selection is subordinate. Although we cannot agree with Mr. Mivart's teleological views, we do agree with him as regards the importance to be attached to these internal causes of change. In one of the most interesting chapters

(as they always must) amidst a frequently varying set of external agencies, also appear as the principal causes of that tendency to undergo progressive development which has now been so fully demonstrated amongst lower organisms, but in whose existence Mr. Darwin did not believe [1].

This absence of a belief in progressive development, as well as some untenable doctrines concerning the rate of change in lower organisms, seem almost wholly traceable to the manner in which Mr. Darwin has approached the general doctrine of Evolution. In working out his theory, he has principally directed his attention to higher organisms, and though his labours have been crowned with so much success—though with the intuition of genius he has unravelled one of the most complex problems which the evolutionist has to face—still it is in this sense principally that he can be considered an evolutionist. He is an evolutionist

of his book ('Genesis of Species') Mr. Mivart calls attention to the various resemblances existing in nature between the structures of organs or parts whose presence only in some of the more specialized forms of totally distinct types, leads us to believe that they must have been formed in a wholly independent manner. The recurrence of similar structures in this way seems to show an inherent fitness in them for the performance of certain functions under certain conditions, and at the same time to demonstrate the ever-active influence of two great determining causes of form and structure.

[1] In the main the tendency is one towards 'progressive' development, though the general tendency to increase in complexity is frequently interrupted in the most irregular manner by retrograde changes, answering to processes which I have classed under the head of Analytic Heterogenesis.

only in part; and he has attempted to work from above downwards, whilst in this philosophy the safest and most trustworthy route is certainly to be found by working upwards from simpler to more and more complex organisms. It is not surprising, therefore, that under these circumstances—amidst the conflict of new views with old beliefs—some inconsistencies should have crept into his writings. And, as it appears to me, nothing could be more thoroughly opposed to the spirit of the Evolution philosophy than Mr. Darwin's hypothesis of Pangenesis [1].

All the facts which this contradictory hypothesis was destined to explain can be much more simply interpreted (so far as they are to be explained at present) by Mr. Herbert Spencer's counter hypothesis concerning 'physiological units'—the truth of which, in an enlarged sense, has now, I think, been definitely established by the results of numerous experiments. And if Mr. Spencer should be induced, by the evidence which has now been brought forward, to believe in the continuous independent formation of living matter in the present day, he will, perhaps, be led to modify some of his statements concerning the absence in living matter of internal proclivities to organization; and would thus obliterate the chasm which at present separates us, and prevents my more complete accordance with his biological views. For a belief in the continued occurrence of Archebiosis and of

[1] See p. 98.

Heterogenesis would necessarily carry with it modified views concerning internal tendencies, and the mode and rapidity of origin of specific characters—and this, I venture to think, is the kind of rectification which biologists will soon begin to demand from the most distinguished exponent of the Evolution philosophy.

CHAPTER XXIV.

GENERAL CONSIDERATIONS: CONCLUSION.

Test of True Theory. Explanation of present existence of Low Organisms. Limits of Ephemeromorphism uncertain. Difficulties concerning some Lower Organisms. Comparative fixity of Higher Types. Nature of Foraminifera. Apparent Persistence of certain Forms through long Ages. Another Explanation of same Facts. Professor Huxley on 'Persistent Types.' Different Explanation of Facts. Similarity of independent Forms might be expected. Demands upon Time required by Mr. Darwin's Hypothesis. Sir Wm. Thomson's Views. These more in accordance with New Theories. Interpretation of Palæontological Records. Similarity amongst Lower, and dissimilarity amongst Higher Organisms in different Ages. No Continuous Progression. Imperfections of the Geological Record. Mode of Origin of Human Race. Questions concerning Superiority of Type. The Insect and the Fish. Stress laid upon Complexity of Structure rather than of Function. Subdivisions of Vertebrate Type. Evidences of Divergence amongst them. Many of them not necessary Precursors of Man's Appearance. Increase in Development of Brain. First Social Habits. Mr. Wallace on results of Comparative Stability of Man's External Form with Progressive Development of Brain. Prejudices concerning Origin of Human Race. Their Childish Nature. Future Prospects. Conclusions.

AS Mr. Wallace has very truly said, 'There is no more convincing proof of the truth of a comprehensive theory than its power of absorbing new facts, and its capability of interpreting phenomena

which had been previously looked upon as unaccountable anomalies[1].' We have already pretty fully pointed out to what an extent the views advocated in these volumes are confirmed by the wide application of such tests. A few apparent anomalies, however, still remain for further consideration, which, although quite irreconcilable with generally-received doctrines, may, perhaps, be cleared up by the light derived from new views.

How, it may be asked, is the possession by living things of an internal tendency to develop, consistent with the existence of such multitudes of low organisms in the present day and in past geologic ages? And how is it consistent with what we know concerning the Persistence of Types?

We have already[2] pointed out the almost insuperable difficulties in accounting for the present existence of lowest organisms in accordance with the old hypothesis. This, as expressed by Mr. Darwin[3], is, that 'all the living forms of life are the lineal descendants of those which lived long before the Silurian epoch.' On the other hand, what has been made known in this work now relieves us from all difficulties concerning the existence of very low organisms at the present day—our Ephemeromorphs. We may safely discard all notions concerning their pre-Silurian pedigree, and

[1] 'Contrib. to the Theory of Nat. Selection,' 1870, p. 45.
[2] See pp. 103–108.
[3] 'Origin of Species,' 5th ed., p. 578.

may regard them as more or less recent products of Archebiosis and Heterogenesis.

We do not pretend to fix the limits of ephemeromorphism and to say, on all sides, where true species begin—though we may expect that much light will be thrown upon this subject by subsequent workers. It is only natural, however, to suppose that the transitions from heterogenesis and metamorphism to homogenesis and specific fixity should be more or less gradual; so that many forms will have to remain, as it were, upon the border-land. Ascending development amongst the ephemeromorphs tends to lead to the production of more complex and less variable forms. But even long after the first rudiments of a sexual generation have been arrived at, and the origin of 'species' has commenced, we might expect that the comparatively unspecialized matter of which such organisms are composed, and the comparative weakness of the internal conservative principle (essentially based, as it is, upon heredity and complexity of organization), would permit of their undergoing marked variations from time to time, either under the influence of changed external conditions or by virtue of 'spontaneously' initiated internal changes [1].

[1] Mr. Darwin's view is directly opposed to this; for instance, after speaking of fresh-water productions, and the fact that many of them are low in the scale of nature, he adds (loc. cit., p. 467):—'We have reason to believe that such *low beings change or become modified less quickly than the high.*' And to show that this is no mere casual expression of opinion, we find Mr. Darwin saying in the last page but one of his

How high in the scale, either of plant or of animal life, any extreme degree of variability will extend, we cannot at present say, although we are entitled to expect that it will gradually diminish in extent as the complexity of organization increases[1]. It may well be, that after even comparatively rudimentary degrees of organization have been evolved, a condition of comparatively stable moving equilibrium is attained, so that—the conditions of life remaining the same—the organism may no longer present anything more than a mere dwarfed tendency to further differentiation[2]. Its internal tendencies may have more or less satisfied

work:—'During early periods of the earth's history, when the forms of life were probably fewer and simpler, the rate of change was probably slower; and at the first dawn of life, when very few forms of the simplest structure existed, the rate of change may have been slow in an extreme degree.' Now, although such views are quite consistent with Mr. Darwin's exaggerated belief in the all-powerful efficacy of Natural Selection as a producer of change, they are wholly opposed to the almost universal observation of naturalists, and just as antagonistic to the legitimate *à priori* deductions of the Evolution philosophy.

[1] See Herbert Spencer's 'Principles of Biology,' vol. i. pp. 192-200.

[2] Such organisms may, however, become more amenable to the modifying influence of changes in external conditions at periods when their vitality is temporarily lowered—just as a top may be made to totter by a slight touch when its axial rotations are becoming slow, though it would have been apparently unaffected by a similar touch made at a time when it was spinning rapidly. Rapid growth indicates rapid molecular movements in the things which grow. In the more putrescible infusions the molecular movements are probably extremely rapid; enormous quantities of Bacteria are quickly produced, which, owing to their frequent fission, seem never to grow or increase in size. It is almost impossible to modify the forms which appear in such a solution. (See pp. 137 and 502.)

themselves by the establishment of a moving equilibrium, just as the internal tendencies of crystallizable matter are satisfied (though here to a much greater extent) as soon as it has assumed its appropriate crystalline form. When a certain complexity of structure has been attained, it may be that things are much as they have been represented to be by Mr. Darwin, Mr. Herbert Spencer, and others. Many organisms may not be prone to vary, unless under the influence of 'spontaneously' induced, quasi-accidental changes within their own economy, or unless subjected to the disturbing influence of new conditions, or to the more powerful and certain action of Natural Selection, aided by 'use and disuse' and other agencies.

Multitudes of facts bearing upon the apparent persistence, without variation, of particular species through comparatively long periods, ought to impress us with the possibility of the existence of such a limitation to the internal proclivities towards higher organization which simpler living things display. Some of these facts are alluded to by Mr. Darwin when he says[1]:—'It has been argued that, as none of the animals and plants of Egypt of which we know anything have changed during the last 3000 years, so probably none have been modified in any other part of the world. The many animals which have remained unchanged since the commencement of the glacial period would have been an incomparably stronger case, for these have

[1] 'Origin of Species,' 5th ed., p. 148.

been exposed to great changes of climate and have migrated over great distances[1]; whereas in Egypt during the last 3000 years the conditions of life, as far as we know, have remained absolutely uniform.' Mr. Darwin then adds:—'The fact of little or no modification having been effected since the glacial period would be of some avail against those who believe in an innate and necessary law of development, but is powerless against the doctrine of Natural Selection or survival of the fittest, which implies only that variations or individual differences of a favourable nature occasionally arise in a few species, and are then preserved.' The facts above cited are, however, not at all inconsistent with a belief in progressive development, when this belief is limited in the way I have mentioned. And there are, moreover, very excellent reasons for believing that the structures of higher organisms are much less easily modified than those of lower organisms. Mr. Spencer sums up the results of his interesting speculations on this subject in the following words[2]:—'Without assuming fixity of species, we find good reason for anticipating that kind and degree of stability which is observed. We find grounds for concluding *à priori* that an adaptive change of structure will soon reach a point beyond which further adaptation will be slow; for concluding that when the modifying cause has been but

[1] Mr. Spencer would perhaps suggest that their migration may have been one of the means of protecting them from such changes of climate.

[2] 'Principles of Biology,' vol. i. p. 199.

a short time in action, the modification generated will be evanescent; for concluding that a modifying cause, acting even for many generations, will do little towards permanently altering the organic equilibrium of a race; and for concluding that, on the cessation of such cause, its effects will become unapparent in the course of a few generations.'

The question, therefore, as to the mode of explanation of the existence of very simple organisms at the present day, merges in an almost insensible manner into that concerning the explanation of the existence of so-called Persistent Types.

The Foraminifera, for instance, are organisms which, from the absence of all sexual distinctions, and from the extreme simplicity of their body-substance, ought, in spite of the notable complexity of the shell-like structures which many of them inhabit, undoubtedly to rank amongst the ephemeromorphs. The animal substance of these organisms differs little from that of Amœbæ, Arcellinæ, and other simple Rhizopods; and, moreover, we are distinctly informed by Dr. Carpenter that any such definite assemblages of individuals as are usually included under the word 'species' do not exist amongst them. He says:—'The range of variation is so great amongst *Foraminifera* as to include, not merely the differential characters which systematists, proceeding upon the ordinary methods, have accounted *specific*, but also those upon which the greater part of the *genera* of this group have

been founded, and even in some instances those of its *orders*. . . . The ordinary notion of *species*, as assemblages of individuals marked out from each other by definite characters that have been genetically transmitted from original prototypes similarly distinguished, is quite inapplicable to this group; since, even if the limits of such assemblages were extended so as to include what would elsewhere be accounted genera, they would still be found so intimately connected by gradational links that definite lines of demarcation could not be drawn between them. . . The only natural classification of the vast aggregate of diversified forms which this group contains, will be one which ranges them according to their direction and degree of divergence from a small number of principal family-types[1].'

These conclusions to which Dr. Carpenter has arrived, are also thoroughly in accordance with the views of his coadjutors, Messrs. Parker and Rupert Jones; and they are strikingly similar to the conclusions which Cohn was compelled to adopt[2] after an examination of the products to which Protococcus gave rise—comprising as they did the most widely differing forms of Phytozoa, which were actually seen by him to be derived from one another by direct processes of transformation. If, therefore, the mere comparison of the separate forms them-

[1] Carpenter's 'Introduction to the Study of the Foraminifera,' 1862, p. x.

[2] See *Appendix* D, p. lxxxiii.

selves has led Dr. Carpenter and his coadjutors to come to such conclusions, it seems more than probable that we are entitled to rank these organisms amongst the ephemeromorphs. Dr. Carpenter finds, moreover, that similar types and similar varieties from these types are to be met with in geological formations existing as far back as the upper Triassic rocks; and he therefore comes to the usual conclusion from such facts—viz. that the forms of Foraminifera existing at the bottom of our ocean in the present day, are the lineal descendants of those simpler forms which lived, ages and ages ago, in the oceans existing when the Triassic rocks were being formed.

This view, however, as I have previously stated[1], seems to me much less probable than the supposition which now lies open to us. We may imagine, for instance, that the sum-total of conditions at the bottom of a deep ocean have probably undergone very slight variations since Triassic times[2], and that the lower forms of

[1] See p. 103.

[2] And, as before stated (p. 502), many facts seem to show that in the very lowest organisms the 'internal forces' are so powerful as to make these organisms comparatively impervious to the influence of any ordinary amount of difference in external conditions. How else are we to account for the similarity of Bacteria and Torulæ over all parts of the world and under most various conditions; and for the fact that the most rudimentary Fungi, Algæ, and Lichens are also met with in all regions of the earth—presenting modifications, it is true, amongst themselves in each separate place, though it seems to make comparatively little difference whether this place be in the neighbourhood of the equator or of the pole?

life (including the Foraminifera) which were to be found in such situations at that time had been produced by Archebiosis combined with Heterogenesis; so that if the conditions have been always so similar in the regions in question, we might expect that more or less similar forms would have been constantly arising by Archebiosis and Heterogenesis, and that these would also go through comparatively similar developmental changes.

The theory of 'lineal descent' is most unlikely to be true; because, as we have already pointed out, the improbability is extreme that such low and unspecialized forms should have existed through so many ages without undergoing any appreciable advance in complexity of organization. On the other hand, many of the ordinary forms of Foraminifera may have been produced originally, as well as in all subsequent periods, by the occurrence of comparatively common heterogenetic changes: and such a view is all the more easy for us to adopt now that we know how readily Arcellinæ are still engendered from the substance of dying Rotifers and other low organisms [1].

Almost similar modes of reasoning are, moreover, now applicable in order to explain the existence through successive geological epochs of many other lower forms of life. Concerning the facts, Prof. Huxley says [2]:—'Certain well-marked forms of living beings have existed through enormous epochs, sur-

[1] See p. 486. [2] 'Proc. of Roy. Instit.,' vol. iii. p. 151.

viving not only the changes of physical conditions[1], but persisting comparatively unaltered, while other forms of life have appeared and disappeared.' 'Such forms,' he says, 'may be termed "persistent types" of life; and examples of them are abundant enough in both the animal and the vegetable worlds.' He then cites the following examples:—'Amongst plants, for instance, ferns, club-mosses, and coniferæ, some of them apparently generically identical with those now living, are met with as far back as the carboniferous epoch; the cone of the oolitic *Araucaria* is hardly distinguishable from that of existing species; a species of *Punis* has been discovered in the Purbecks, and a walnut (*Juglans*) in the cretaceous rocks. All these are types of vegetable structure abounding at the present day; and surely it is a most remarkable fact to find them persisting with so little change through such vast epochs. . . . Every sub-kingdom of animals yields instances of the same kind. The *Globigerina* of the Atlantic soundings is identical with the cretaceous species of the same genus; and the casts of lower Silurian *Foraminifera*, recently described by Ehrenberg, assure us of the very close resemblance between the oldest and the newest forms of many of the *Protozoa*. . . Amongst the *Cœlenterata*, the tabulate corals of the

[1] Which, in accordance with the 'unformatarian view of teluric conditions, so far as geological time is concerned,' he believes to have 'varied within but narrow limits: so that even in Silurian or Cambrian times the aspect of physical nature must have been much what it is now.'

Silurian epoch are wonderfully like the millepores of our own seas, as every one may convince himself who compares *Heliolitis* and *Heliopora*. . . . Turning to the *Mollusca*, the genera *Crania*, *Discnia*, and *Lingula* have persisted from the Silurian epoch to the present day with so little change that competent malacologists are sometimes puzzled to distinguish the ancient from the modern species. *Nautili* have a like range, and the shell of the liassic *Loligo* is similar to that of the "squid" of our own seas. Among the *Annulosa*, the carboniferous insects are in several cases referable to existing genera, as are the *Arachnida*, the highest group of which, the scorpions, is represented in the coal by a genus differing from its living congeners only in the disposition of its eyes.' Now, without dwelling at present upon the almost similar persistence of various representatives of the different vertebrate groups, it seems to me that the 'persistence' of many of the plants and of the lower invertebrate types is much more explicable on the assumption of successive evolutions of more or less similar forms from similar starting-points under the influence of like conditions, than on the assumption that such changeable forms should have reproduced their like without any very appreciable alteration through such vast and unrealizable epochs of time[1]!

[1] I am glad to find that more or less similar views have also been expressed by my colleague, Dr. Grant. He says:—' The existing races, which alone concern us here, are not descended from each other, although from more simple common ancestry, and they do not, there-

The opposing notion that all the forms of life which at present exist — including the structureless Amœba and the insignificant Mucor which now springs up on decaying substances—are direct lineal descendants of organisms which lived, ages before the birth of man, in far distant pre-Silurian epochs, seems to me opposed to all reason, from the point of view of the evolutionist[1]. But that a great amount of similarity should exist between the earliest forms of organisms which have appeared in intervening epochs and those which exist at the present day—whether amongst Cryptogams, Foraminifera, Mollusca, or other animals—is not so much to be wondered at when we recollect that the starting-point or primordial constitution of living matter has ever been generically the same, that the internal determining causes of change have been constant, and that the sum-total of surrounding conditions have probably not been more various than we might be warranted in

fore, form the links of a continuous chain from the monad to the man. They are all equally co-existent, independent, and unconnected with each other, like the extreme peripheral buds of a tree of life, whose base is largely concealed or consumed in the earth, but whose more recent branches can be readily traced through all the surviving fossiliferous strata of the globe. Such ramifying trees of life, however, have never ceased to originate and develop *de novo* in the same mode as the first, since the first found a suitable habitat; and it is neither necessary nor philosophical to assume that any animal type had a different mode of origin from that of another—the durability of a type being the best proof of its natural origin.' ('An Outline of Recent Zoology,' 1861, p. 9.) See also an allusion to the more recently published views of Mr. G. H. Lewes on the same subject, at p. 75.

[1] See p. 589.

believing, from the differences exhibited amongst the several essentially similar types which have presented themselves through a countless succession of geologic ages. Looked at broadly, it is not contrary to what we might expect, if we find that the most marked and long-continued similarity exists between some of the lowest forms of life preserved in the fossil state, which, being tenants of deep seas and oceans, have all along been exposed to almost similar conditions[1]. The constitution of primordial living matter in the Silurian epoch, in all probability, essentially resembled the primordial living matter of to-day; and if the conditions existing at the bottom of oceans are also similar, what could be expected but that forms which are comparatively little removed from the primordial living matter of those ages, should be similar to those which are removed to a like extent in the present day, and to those which have been similarly removed in all intervening epochs? No more competent authority exists than Dr. Carpenter concerning the structure of Foraminifera, and we have his deliberate statement that[2] 'there is no evidence of any fundamental modification or advance in the Foraminiferous type from the palæozoic period to the present time.' And further

[1] Mr. Darwin says ('Origin of Species,' p. 409):—'Consider the prodigious vicissitudes of climate during the pleistocene period, which includes the whole glacial epoch, and note how little the specific forms of the inhabitants of the sea have been affected.'

[2] 'Introduction to the Study of the Foraminifera,' 1862, p. xi.

on he says:—'The Foraminiferous Fauna of our own seas probably present a greater range of variety than existed at any previous period; but there is no indication of any tendency to elevation towards a higher type.'

Again, it has been not unreasonably urged by some persons that if the organic world had been really evolved by the agencies which Mr. Darwin seems to believe almost exclusively influential, demands would have to be made upon time of so exorbitant a nature as to frighten even the most liberally disposed geologists and physicists. And this is believed by many to be a matter of some moment. Sir Wm. Thomson[1], indeed, has given reasons for the opinion that no such vast periods of time can have elapsed since the surface of our earth became sufficiently cool to permit of the presence of living things. He thinks this stage of the Earth's history cannot have been attained more than 400,000,000 of years ago. The subject is, perhaps, one in which the data may be insufficiently known to permit of a reliable calculation being made, though no one could speak with higher authority on such a problem than Sir Wm. Thomson. We may, however, confidently state that the alarming demands for very vast periods of time made by biological evolutionists would be materially diminished if views like those which we have advanced were commonly entertained[2].

[1] 'Trans. of Geolog. Soc. of Glasgow,' vol. iii.

[2] See p. 429, note.

Moreover, we firmly believe that the exceedingly imperfect and fragmentary palæontological record may be much more intelligibly read in accordance with our views than with those which are at the present time most commonly accepted. The task itself, however, we must leave to other and more competent persons. We will merely state that the continued existence of low types throughout the geologic strata from the Silurian system upwards, and, amongst higher types, the constant admixture of previously known forms with others altogether new, will be found quite consistent with the notion of a continual surging up through all geologic time of freshly-evolved, lower forms of life—representatives of which, as they become more and more highly organized, mix, in successive epochs, with those of their predecessors which still remain. Thus there would always be a continual striving onwards of old and new alike, towards those highest goals which the direction of development and the sum-total of surrounding conditions at the time rendered possible.

These considerations would, moreover, lead us to expect that, whilst more or less similarity would be likely to exist between the lower forms of life which have existed at different periods of the Earth's history, more and more divergence might be encountered amongst much higher aquatic or aerial types whose ancestors may have lived through long geologic ages. Amongst such forms, considerable diversity may be

induced by the slow accumulation of minute differences[1]. So that if the descendants of similar organisms (derived perhaps from totally independent stocks) have been exposed to notably different external conditions in different ages; or if in any of them modifications have otherwise arisen, the forms ultimately produced along such lines of development may be widely different from one another, although belonging to similar types.

And at different periods in the earth's history, specializations, now of one type and now of another, have been more and more manifest or dominant. In the Silurian epoch, strange crustacean Trilobites abounded in all the seas. In the Devonian epoch fishes of a remarkable structure were most plentifully represented; whilst the same may be said of the cup-like Encrinites in the earlier Carboniferous period, though in the later portions of this epoch they were altogether thrown into the shade by that vast tropical vegetation from which we now derive our supplies of coal. In the Oolitic period, or so-called 'age of reptiles,' we have a most remarkable abundance of Saurian forms, and the Amphibian type reached its highest development. Huge Ichthyosauri and Plesiosauri swam in the lakes and rivers, whilst strange and gigantic

[1] As Mr. Darwin remarks (loc. cit., p. 570):—'The mind cannot possibly grasp the full meaning of the term of even ten million years: it cannot add up and perceive the full effects of slight variations accumulating during an almost infinite number of generations.'

winged Lizards mounted into the air. Whilst in the later Tertiary period we find the Mammalian type exhibiting remarkable divergences from previously-existing forms; first by the appearance of innumerable, huge Mastodons, Megatheriums, and other unwieldy denizens of the ancient forests and plains; and subsequently by the gradual modification of one of the ramifications of the Quadrumanous order, into those beings from whom primeval Man himself may claim to have been evolved.

These several types of life, however, which have from time to time become more and more specialized, do not in any sense represent the members of one progressive series. They are rather the products of different evolutional divergences, taking place now in one direction and now in another.

Our knowledge of the various living forms which have existed in past ages is, indeed, of the most fragmentary character: first, on account of the unequal or imperfect manner in which the several forms may be represented in the strata pertaining to the period; secondly, on account of the extremely limited nature of the explorations which have been made in these imperfectly representative strata; and, thirdly, because so many parts of the record are absolutely inaccessible to us—nearly all beneath the Silurian system having been blotted out by time, whilst those two-thirds of the earth's surface in which the remaining strata are to be found are now covered over

by seas. Mr. Darwin says[1]:—'For my part, following out Lyell's metaphor, I look at the geological record as a history of the world imperfectly kept, and written in a changing dialect; of this history we possess the last volume alone, relating only to two or three countries. Of this volume, only here and there a short chapter has been preserved; and of each page only here and there a few lines.' Such as it is, however, the record seems to show very plainly that there has been nothing approaching to a continuous progression terminating in the Mammalian type. Vertebrata in the form of fishes as high as any existing at the present day, have been in existence since the time when the upper Silurian rocks were deposited. Whilst at different intervening periods in the earth's history, now one, now another of the invertebrate forms of life have been in the ascendant, associated, perhaps, with representatives of some highly developed and divergent branch of the vertebrate tree. Till at last—as it were accidentally—on the top of one of these diverging branches, some of the branchlets pertaining to the quadrumanous order began to undergo modifications which terminated in the evolution of the immediate ancestors of the primeval representatives of our race.

It is, therefore, illegitimate and unscientific to regard all preceding forms of life as belonging to types lower than our own, or to suppose that they have been the necessary precursors of our advent.

[1] Loc. cit., p. 384.

Most of us are apt complacently to regard ourselves as representatives of the highest type of life: and to a certain extent this is true. Although it is often exceedingly difficult, and in some cases impossible, to decide which is the higher and which is the lower of two forms of life whose type is different. Supposing, for instance, a question should arise as to the relative superiority of the fish and the insect; our thoughts may quite legitimately take the directions recently indicated by an able writer in the 'Quarterly Review,' who says[1]:—'On the one hand we may ponder over the dreary simplicity of a fish's life, the monotony of its daily swim, the low character and even small amount of nervous energy required to move its uniform masses of muscle, and the feeble working of its diminutive brain—limited apparently to the stirring up, through rough and gross sensual perceptions, of a turbid consciousness, which the accumulation of even years of experience can hardly mould into anything like intelligence. Even in performing that duty which generally calls forth the highest cerebral activity, viz. the care of the young, the greatest effort of the fish is perhaps to construct a nest of the rudest kind.' But, 'Turning from these cold and flabby creatures to the gifted bee, and meditating on its bright and varied life—on those wonderful exhibitions of its power and skill which never fail to excite the admiration of mankind, and on its finely-wrought and compact

[1] Art. on 'Higher and Lower Animals,' Oct. 1869, p. 383.

organization put to use in the facile accomplishments of difficult and delicate tasks, we begin to think it not unnatural to rank so full a life above that of the plainer vertebrate. . . Such an advocate would, moreover, point to the fact that the bee's life is as short as it is bright; that it has little time to learn, little opportunity of accumulating experience either for itself or for its off-spring; that it is, so to speak, a baby-bee, and one of a long line of baby-bees: and would argue that were a bee long-lived, and the race to continue long-lived through many generations, there would be no longer any disputes about the reason and instinct of bees.' Thus, from the point of view of the functions performed rather than of structural possibilities, we should undoubtedly be led to regard the bee as a much higher animal than the fish. And as the same writer continues: —'If we were to put ourselves entirely on one side, and try to look at animal life as a thing in which we had neither part nor lot, we should of course, in attempting to fix the rank of any being, be guided almost exclusively by the range and complexity of the duties the creature was enabled to fulfil. We should use function almost by itself as a test of worth, and should look upon structure as simply the means to an end. . . . Dignity of function, springing as it does out of intricate and finished machinery, must, when we look at animals apart from ourselves, form the standard by which rank in life can be judged.'

We do not, however, usually put ourselves on one

side in this way, so that the question is generally looked at more from the point of view of structure than of function. And we are, then, for the most part content to regard the vertebrate type as the highest in the whole animal creation, although as the same writer says, 'Our willingness to do so arises simply from the fact that that is the type to which we ourselves belong. But,' he continues, 'we have no right to reflect our own glory on to the type according to which we happen to be framed. Such an honour to the mere type itself is altogether an arbitrary assumption. Admitting that we stand at the head of creation, that we are the highest product of organic life, we can rightfully claim that position solely by virtue of our many powers and resources, solely by reason of our mastery over the circumstances of life. . . . But we have no warrant for the assumption that there is any absolute necessary causative connection between the scope and quality of the work we do, and the broad features, either vertebrate or mammalian, of the plan upon which our bodies are framed. . . . It so happens that we, and with ourselves many other vertebrate animals, do exhibit finer qualities and live a fuller life than do any other creatures; but proof is wanting that qualities as fine or even finer, that a life as full or even fuller, might not have been thrown round and worked into some rough ground-plan other than ours. The mammal does not differ from the fish more than does the crafty lobster from the tiny, one-eyed crustaceans that breed in our fresh-water

pools; as much power has been needed to raise the one as the other, and there is no *à priori* reason why the forces which have grafted on the insect type the features and powers of an intelligent bee should not go on to work the same type into something possessing powers and qualities as full, as great, and as varied as those which we ourselves possess.'

The vertebrate type itself is, moreover, subdivided into many subordinate types, each of which goes along its own lines of development and differentiation, so that there is no trace of any single and general progression towards the perfection of that mammalian branch, of which Man forms the head and crown. As we have already stated, Fishes, Amphibia, and Reptiles of the most divergent forms have been abundantly produced, departing in the most striking manner from one another and from the general vertebrate plan. Whilst amongst Birds also, such specializations of the respiratory system, of the organs of locomotion, and of the integumentary system have taken place, that the type on which Man's body has been evolved has, in these respects, been left far behind. Doubtless, therefore, if the conditions had been suitable, the progenitors of the quadrumanous race might have given rise to all that has appeared along this line of development far back in the depths of geologic time—and wholly irrespective of the multitudes of aberrant forms of vertebrate life which have flourished since the Silurian epoch.

There is, indeed, no small amount of evidence de-

ducible from the history of the life of the globe antecedent to the advent of Man, tending to prove that many of the above-mentioned developmental divergences cannot be regarded as constituting so many necessary preliminary series. The palæontological records, so far as they have been discovered, would rather encourage a belief that we happen to live during one of those great phases in the earth's history in which an aberrant type, having within itself vast and altogether peculiar capacities for improvement, has, on account of the high development of these capacities, overrun the earth. Those mysterious powers and natural tendencies, which formerly sufficed to produce the great fish-like lizards and crocodiles, and which, amongst birds, have expended themselves in the perfection of an elaborate respiratory system and in the production of related changes in their integumentary system and organs of locomotion, seem, in the case of Man and of the race from which he has been developed, to have been expended in the production of much less obvious external changes, although these have been accompanied by the most important internal changes leading to the gradual elaboration of the Brain, or principal Organ of Mind.

An increased development of the Brain, however initiated, and even when it gave to primæval Man mental powers very slightly in excess of those of the man-like apes, would, after a time, as Mr. Wallace has ably shown[1],

[1] 'Contributions to the Theory of Natural Selection,' 1870, p. 319.

almost inevitably tend to give him that power over natural products and forces which in the course of ages has enabled him to make these forces subservient to his own wants in a gradually increasing degree. The altered mode of life also, which must have been thus induced at a very remote period, is conceived by many to have been sufficient—when acting through enormous periods of time, and when supplemented by the successively-acquired powers of speaking, writing, and printing—to account for the vast progress that has since been achieved both in his intellectual and in his moral nature[1].

But for such a continuous development of brain to have taken place, with co-ordinate increase in mental and moral attainments, we must naturally have recourse to long periods of time—though not longer perhaps than the period which we are now able to claim. Thus, as Mr. Wallace says[2]:—'We can with tolerable certainty affirm that man must have inhabited the earth a thousand centuries ago; but we cannot say that he positively did not exist, or that there is any good evidence against his having existed, for a period of ten

[1] Others, however, do not suppose that the moral nature of Man can be thus naturally accounted for. Mr. Wallace, as well as Mr. St. George Mivart (chap. ix), believe that at this particular stage of organic evolution some supernatural interposition must have taken place. Mr. Wallace, indeed, cannot believe that some of the mere bodily peculiarities of Man have been produced by the undisturbed action of natural agencies. (See his 'Contributions to the Theory of Natural Selection,' 1870, p. 333.)

[2] Loc. cit., 1870, p. 303.

thousand centuries. We know positively that he was contemporaneous with many now extinct animals, and has survived changes of the earth's surface fifty or a hundred times greater than any that have occurred during the historical period; but we cannot place any definite limit to the number of species he may have outlived, or to the amount of terrestrial change he may have witnessed.'

Mr. Wallace has, moreover, adduced a number of very cogent reasons for believing that such creatures as the ancestors of man must have been, would, at a certain stage in their history, have ceased to undergo any notable variations in external conformation, although a steady progress might continue to take place in the developmental organization of the brain, leading to a progressive increase of mental power. He says[1]:—'From the moment when the first skin was used as a covering, when the first rude spear was formed to assist in the chase, when fire was first used to cook his food, when the first seed was sown or shoot planted, a grand revolution was effected in nature, a revolution which in all the previous ages of the earth's history had had no parallel; for a being had arisen who was no longer necessarily subject to change with the changing universe—a being who was in some degree superior to nature, inasmuch as he knew how to control and regulate her action, and could keep himself in harmony with her, not by a change in body, but by an advance

[1] Loc. cit., pp. 325 and 317.

of mind. . . . Every slight variation in his mental and moral nature, which should enable him better to guard against adverse circumstances and combine for mutual comfort and protection, would be preserved and accumulated[1]; the better and higher specimens of our race would therefore increase and spread, the lower and more brutal would successively give way and die out; and that rapid advancement in mental organization would occur, which has raised the very lowest races of man so far above the brutes (although differing so little from some of them in physical structure), and, in conjunction with scarcely perceptible modifications of form, has developed the wonderful intellect of the European races.'

Surely, however, we are called upon to witness a strange perversity of human reason when many of those who have become the heirs of such higher development attempt, more or less indignantly, to repudiate its origin—when, on the strength of the high elaboration of those faculties which they have inherited from simpler and less polished predecessors, they are now eager

[1] Principally from the inheritance of functionally-induced changes; for, as Mr. Spencer says ('Principles of Biology,' vol. i. p. 469), 'though Natural Selection acts freely in the struggle of one society with another, yet among the units of each society its action is so interfered with that there remains no adequate cause for the acquirement of mental superiority by one race over another, except the inheritance of functionally-produced modifications.' And, as Mr. Spencer subsequently points out, this view of the case harmonizes well with Mr. Wallace's conclusion, that 'at a certain stage of evolution the brain begins to vary much more than the body.'

to disown all past connection with that great tree of life, to which they are related as most precious fruit. Pitiful, nay, laughable displays! What should we think of the man who stoutly denied his intra-uterine existence, who preferred to believe that he had descended direct from the clouds (albeit fashioned after the model of an ape), because in his childhood his nurse had told him such a story? And yet a person so credulous—one who persisted in believing the story he had first heard and in disbelieving all those who were prepared to prove its untruth—would appear to many dispassionate onlookers to be not more unreasonable than are those who try to disown all kinship with the products of surrounding nature, and prefer to regard themselves as degenerated representatives of some higher modes of being.

The early history of the individual infant is hidden from ordinary mortals, just as completely as the early history of the race is buried in the womb of time. But those who know the history of this intra-uterine existence cannot but feel how strongly it testifies to the real origin and affiliation of the race. These stages of man's existence embody a condensed record of some of the missing pages in the earth's history.

So that to mistrust or ignore all the various kinds of evidence which so plainly appeal to our reason, would be to accustom ourselves to look upon nature as a living lie, and upon reason itself as a faculty destined only to deceive!

We must hope it is not so; and in spite of such demoralizing beliefs must battle on along the path of knowledge and of duty, trusting in that natural progress towards a far distant future for the human race, such as its past history may warrant us in anticipating. For, as Mr. Wallace points out, those natural influences which have hitherto promoted man's progress, 'still acting on his mental organization, must ever lead to the more perfect adaptation of man's higher faculties to the conditions of surrounding nature and to the exigencies of the social state,' so that 'his mental constitution may continue to advance and improve, till the world is again inhabited by a single, nearly homogeneous race, no individual of which will be inferior to the noblest specimens of existing humanity.'

Conclusion.

From what has been recorded in this work, it appears, therefore, that both observation and experiment unmistakeably testify to the fact that 'living' matter is constantly being formed *de novo*, in obedience to the same laws and tendencies as those which determine all the more simple chemical combinations: the qualities which we summarize under the word 'life' being in all cases due to the combined molecular actions and properties of the aggregate which displays them, just as the properties which we include under the word 'magnetism' are due to particular modes of

collocation which have been assumed by the molecules of iron.

Living matter is especially characterized by the complexity of its molecules and their state of continual intestine movement. This peculiarity, as well as other related qualities, make the simplest aggregates of such matter especially prone to undergo those secondary structural rearrangements which all plastic and homogeneous masses of matter are liable to exhibit. And although in the case of living matter, these re-arrangements manifest themselves by producing what we call 'organization,' still the forms and structures which many of the lowest organisms tend to assume are entirely referrible to the polarities of their molecules—just as the forms of crystals are the results of similar, though simpler, polarities.

And, speaking generally, the complexity of 'organization' attainable by the lower animal forms gradually tends to increase as the masses of matter from which new forms are to arise increase in size—owing apparently to the multiplication of effects that may be induced by the production of several series of molecular rearrangements within the larger aggregates. These rearrangements (developmental changes) often take place rapidly and without appreciable increase in bulk of the mass which undergoes them; and the dissimilar changes which may be seen to take place in different masses are attributable to the existence of different initial states of molecular composition. The changes

progress, however, in each case, till a condition of moving equilibrium is established between the sum total of molecular actions taking place within the living aggregate and the forces of its environment.

The power of undergoing spontaneous division (fission or gemmation) which is manifested by living matter, and upon which all the phenomena of 'reproduction' depend, is apparently one of its most fundamental properties—though it is itself a result of that molecular mobility and complexity to which we have previously referred.

And it is this same molecular mobility which makes an aggregate of living matter, in the form of a simple organism, very prone to undergo changes in its intimate constitution—either 'spontaneously' or under the incidence of new external forces. Some new conditions may not visibly affect it, others may cause its 'death,' whilst others still may affect it only to such an extent as to bring about some modification of its molecular constitution, which, by reason of an altered 'polarity,' entails a more or less marked transformation of form and structure (Heterogenesis).

Thus the marvellous convertibility of lower organisms, their ability to undergo self-multiplication, and their tendency to become (under favourable conditions) more complexly organized, are all necessary consequences of those physical doctrines concerning 'life' the truth of which has been established by such experiments as have now been recorded.

These myriads of lowest forms of life, multiplying only by processes of fission and gemmation, constitute an inextricably tangled plexus of more or less convertible animal and vegetal forms, which—though often reappearing—are for the most part evanescent and transitory states, either of comparatively new-born living matter, or of portions of matter which have become individualized by heterogenetic processes occurring in the substance of the higher forms of life. But howsoever derived, they constitute a vast assemblage of 'Ephemeromorphs,' amongst which Heterogenesis occurs almost as frequently as Homogenesis.

Gradually, however, the first traces of those processes of 'conjugation' and of internal gemmation begin to manifest themselves, which subsequently become perfected into 'sexual' modes of reproduction.

Connected series of transformations tend to occur, in the last of which by a sexual reproductive act a fecundated germ is produced, which in its development goes through a similar series of transformations; or else animal organisms of a decidedly sexual type are more directly formed by the 'vital crystallization' of larger and more specialized egg-like aggregates of living matter, which have been elaborated during the life of some lower vegetal organism.

But when animal or vegetal organisms manifesting that cyclical homogenesis which is known as 'alternate generation,' appear upon the scene, and with them those simpler allies (formed from large germs) which

undergo a direct process of development, we first begin to obtain such regularly-recurring and definite assemblages of animal and vegetal forms as are usually grouped under the name of 'species.'

These 'species' are represented either by solitary hermaphrodite individuals, by two sexually-distinct individuals, or by a series of transitional and derivative individuals of which all the earlier forms are sexless and still retain the power of undergoing processes of asexual multiplication, although in the last form fecundated germs are produced by a true sexual process.

Such organisms have gradually become more highly organized than the 'ephemeromorphs,' and they are to a corresponding extent less immediately capable of being influenced by changes in their environment or other modifying influences. Moreover, as soon as sexual reproduction is initiated and homogenesis becomes the rule, the 'laws of heredity' come into action, and an internal conservative principle begins more and more to take root.

The processes comprehended under what Mr. Darwin has termed 'Natural Selection,' being essentially based upon these laws of heredity, can only come into play as an originator of specific transmutations amongst such forms of life as habitually reproduce by a process of homogenesis. It can only begin to be influential, therefore, when we have passed beyond the limits of that vast and intricately interrelated assemblage of

Infusorial and Cryptogamic Organisms which we have classed under the name 'ephemeromorphs.' Natural Selection as a producer of variation is, indeed, wholly limited to 'species,' such as we now define them.

But amongst specific forms of slight complexity, the influence of Natural Selection as a modifying agent is probably much less important than it is amongst more active and complex animal forms; and in all cases its action in producing change may be assisted by 'spontaneous' internal changes in the molecular activity of certain parts of the organism, or by other internal changes which are more definitely induced by modifications in the sum-total of 'external conditions' acting upon the organism.

Each cause of specific modification, however, whether acting alone or in concert with one of the other producers of internal change, can only come into play in subordination to the ever-potent laws of 'organic polarity,' by which a multiplication of effects is apt to be induced.

An elemental origin of 'living' matter similar to that which takes place at the present day, and in addition all the related heterogenetic phenomena, have probably been taking place on the surface of our globe since the far-remote period when such matter was first engendered.

The countless myriads of living units which have been evolved in different ages of the world's history

must, in each period, have given rise to innumerable multitudes of what have been called 'trees of life,' branching out into animal and vegetal forms of almost inconceivable variety. Myriads of these 'trees,' including all their branches and innumerable ramifications, may have wholly died out during the many vicissitudes of the earth's surface and the long lapse of ever-fruitful ages; though the descendants or ultimate ramifications of some of such trees—dating back to quite different and perhaps far-distant epochs—may still survive upon the earth's surface. How far, however, the roots of any of those trees from which the existing higher forms of life are derived, may have extended back into the depths of geologic time, we are utterly unable to estimate.

Throughout all this life-evolving period of the history of our globe, the progress of 'organization' seems to have been essentially similar. And that this should be so, seems readily explicable by the consideration that living things, both as regards their origin and their subsequent differentiation or development, are the immediate products of ever-acting natural laws or material properties which are probably the same now as they have ever been.

The lower the forms of life—that is the nearer they are to their source—the greater seems to have been the similarity amongst those which have been produced in different ages. On the other hand, the longer any particular tree of life has lived (of which there have been

countless multitudes born in each age), the wider may be the divergence of form presented by the ultimate outgrowths of any two of them, or of outgrowths of similar rank produced from trees which have developed during different ages—especially when the assemblages of organisms, constituting one of these ideal trees, has lived under the influence of any unusual sets of telluric conditions.

How long or when the particular 'tree of life,' from one of the branches of which man was developed, appeared upon the earth, it is utterly impossible to say. The 'vertebrate' grade of organization *may* have been many times attained by ultimate branches of different 'trees of life.'

But physical, chemical, and biological phenomena all compel us to believe that law and order universally prevail, even amidst occurrences which, on account of the complexity of their relations, may seem to have been the result of chance or accident. And equally good reasons also exist for the conviction that the same Forces which are now in action within and around us, have been and are constantly operative throughout the whole universe—everywhere producing the most beautiful and complex results, whose mutual alliance and inter-relations seem to combine in testifying to the existence of one supreme and all-pervading Power of which these results are the phenomenal manifestations.

APPENDIX A.

On some Organisms, and other products of uncertain nature, met with in boiled solutions of Ammonic Tartrate, and also in others containing Ammonic Silicate.

MANY observations and experiments have been made with solutions containing neutral ammonic tartrate and neutral sodic phosphate, both in distilled and in tolerably pure undistilled water[1]. When these solutions have not been boiled, and have been maintained at a temperature of 65°-75° F., they have become more or less turbid in from forty-eight to seventy-two hours, owing to the development and multiplication of myriads of *Bacteria* and *Vibriones*. If they were previously boiled, however, they were not at all prone to become turbid, and might then be kept for a long time, even with free exposure to the atmosphere[2], without any trace of the presence of such organisms; though at any period the solutions may be shown to be eminently favourable media for the development and multiplication of these organisms, since, after they have been purposely brought into contact with a few of them, the solutions speedily become turbid and swarm with *Bacteria* and *Vibriones*. An utter absence of living things of this kind has, however, always been a notable

[1] In the proportion of ten grains of the former and three grains of the latter to one fluid ounce of water.

[2] This freedom from turbidity may be seen, either in open flasks, in flasks closed with or without ordinary air, or in other sealed flasks to which only air which has been filtered through cotton-wool is admitted.

characteristic of those boiled solutions which have been subsequently kept (without such addition) in airless and hermetically-sealed flasks.

But, although the boiled fluids in hermetically-sealed flasks (with or without air) have never been known to become turbid, or to yield *Bacteria* and *Vibriones*, they have nevertheless very frequently been found to present other kinds of organisms. A slight deposit, of a dirty greyish-white or brownish colour, has gradually collected at the bottom of the flask, and this on subsequent microscopical examination has generally been found to contain some organisms, and occasionally bodies of an uncertain nature, intermixed with peculiar amorphous fragments, brown or colourless granules, and a small number of textile fibres of various kinds.

The organisms have been either fungus filaments and spores similar to those represented in Figs. 29 and 36, *Torulæ* such as have been sketched in Fig. 28, various kinds of flagon-shaped bodies of a light brown colour (apparently budding out into filaments and containing blocks of protoplasm within), or else roundish spores of very variable shape and size—some being smooth externally, others rough, and most of them having thick walls.

The fermentability of these solutions seems to be very notably lowered by the process of ebullition to which they have been submitted, and the fungus spores and filaments which subsequently occur appear to grow with extreme slowness (see vol. i. p. 281).

In addition to these unmistakeable organisms (which have in some cases been proved to be really living) obtained from the saline solutions, other bodies have been encountered whose real nature is deemed to be very doubtful.

The first of these is the product called *Sarcina* (Fig. 21), which, since its discovery by the late Professor Goodsir[1], has

[1] See vol. i. p. 286.

been very generally regarded as a real living organism. It has been even admitted into this category by the Rev. M. J. Berkeley, who looks upon it as an unusual form of some one of our common mucors, though he admits[1] that all attempts to develope it into the mucor which it represents have signally failed. This itself is a very noteworthy fact, and one which is extremely difficult of explanation in accordance with the supposition that *Sarcina* is really a living organism. Certain facts observed by Lebert[2] are also well worthy of note. In describing a specimen which he found in prodigious quantities in the fluids vomited by a woman suffering from cancer of the stomach, he says:—'En comprimant les lames de verre entre lesquelles nous examinions ces préparations nous avons été frappé de la sensation sablonneuse, qui ne pouvait après mûr examen, tenir qu'à la dureté des Sarcines ce qui nous faisait supposer qu'elles avaient une enveloppe minérale, peut-être silicieuse.' Without reference to Lebert's more special suggestion, as to the siliceous constituents of *Sarcina*, the fact of the large quantity of mineral matter entering into its composition, and of its occurrence in this and in other cases without any admixture with real fungoid organisms, when taken in conjunction with the failure of all attempts to induce it to undergo any further process of development, suffice to suggest the possibility of its being not a living organism at all, but rather a statical aggregate which grows after the manner of the modified crystalline forms described by Mr. Rainey.

My supposition that *Sarcina* is not a living organism has of late been gradually more and more strengthened. I have been influenced by the following considerations:—1. *Sarcina* has only been seen to undergo a process of growth and

[1] 'British Fungology,' 1860, p. 69.

[2] And quoted by Robin in his 'Végétaux Parasites,' 1854, p. 337.

development[1], never one of spontaneous fission, without which it can have no strict claim to be considered as one of the lowest kinds of living things. 2. The *Sarcina* met with in ammonic tartrate solutions like that from the stomach has always existed amongst the sedimentary deposits. 3. In other ammonic tartrate solutions in which there has been either no *Sarcina* of the ordinary description, or only a small

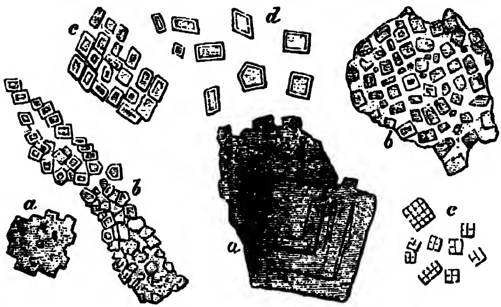


Fig. *a*.

Sarcina **and allied products which have a more obviously crystalloid nature from solutions containing Ammonic Tartrate and Sodic Phosphate. (x 600.)**

a, *a*. Plates partly crystalline and partly amorphous.
b, *b*. Amorphous matter gradually assuming a crystalline form.
c. More perfect group of such crystals.
d. Similar separate crystals.
e. ***Sarcina*** from same solution.

quantity of it, a sedimentary matter has been found, having a very strong general resemblance to *Sarcina*, though the appearance of this has been such as to make it almost certain that it is a kind of modified crystalline rather than a

[1] Similar processes may be seen during the formation of Rainey's 'calculi.' See Chap. xii.

living substance[1]. 4. *Sarçina*, even when obtained from the human stomach, varies considerably, as regards its ultimate pattern or arrangement, as may be seen by reference to Robin's figures[2].

Some of the forms assumed by these masses allied to *Sarcina* are represented in Fig. *a*. Many other intermediate conditions have, however, been observed; and the careful comparison of one with the other has made me strongly of opinion that *Sarcina* is only a member of this series of peculiar, not-living formations.

The next variety of doubtful product met with in the ammonic tartrate solutions is an intricately-tangled spiral-fibre (Fig. *b*), which I have previously described[3], and which I was at one time disposed to think might be a living organism. I am now, however, more inclined to think that it is a very peculiar formation, which should be placed in the category of lifeless rather than of living things. Here also, as with *Sarcina*, though there is good evidence that growth and development take place, there is no satisfactory evidence of the occurrence of reproduction by the spontaneous separation of portions of its own substance. In default of this, and of all other signs pertaining to living aggregates, it cannot be confidently admitted into the same category with them, though it may be just as devoid of all claims to be considered as a crystalline aggregate. It seems, like *Sarcina*, to be an intermediate product, the existence of which is very far from being incompatible with the truth of the doctrines of evolution, or irreconcileable with our notions as to the nature of living

[1] Although I have met with *Sarcina* and its allies ten or twelve times in the solutions above named, they are by no means to be produced at will. In this respect they are just as uncertain as organisms.' On many occasions I have utterly failed to obtain them, although the solutions and the conditions were, so far as I could make them, similar to those which had yielded them on previous occasions.

[2] Loc. cit., Pl. XII. Fig. 1.

[3] 'Nature,' 1870, No. 36, p. 197.

matter[1]. Both products have been seen to appear and increase in amount within closed flasks, as minute whitish masses whose existence at first was, to say the least, not

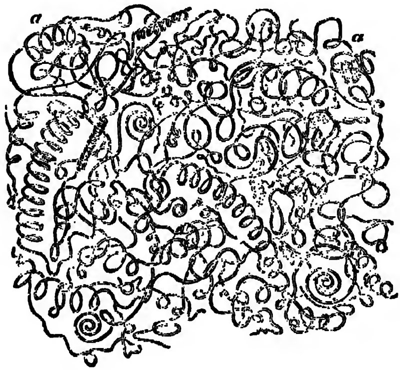


Fig. *b*.

Mass of Spiral Fibre from an Ammonic Tartrate and Sodic Phosphate solution. (x 600.)

recognizable. Both yield no colour reactions with the polariscope (with or without the aid of a selenite plate); whilst in some cases bodies of the modified *Sarcina* type have been

[1] It has been suggested by others that these spiral-fibre masses are accidental products, which, having gained access to the solutions, have been more or less modified therein—portions of the spiral ducts of plants, for instance, or of spider's web. After repeated careful examinations and comparisons, I am still, however, quite unable to adopt either of these views. These particular spiral masses differ wholly from the spiral fibres of plants and all products obtainable from them, and they also differ in many important respects from spider's silk, not only in microscopical characters and in the absence of even slight colour reactions with the polariscope, but also in the complete absence of that silky lustre to the naked eye which still characterizes spider's silk, even after it has been boiled in, and has remained immersed in an ammonic tartrate solution for two or more weeks. Like *Sarcina*, the spiral fibres have been obtained only from slightly *acid* ammoniacal solutions, in which a phosphate was present.

encountered in the same solution with the *Sarcina* and spiral fibres, partly free and partly in intimate connection with the latter (Fig. *e*). The spiral-fibre masses have been seen also in different stages of growth. In some of the solutions of ammonic tartrate minute masses have been seen which were obviously young fibres, the ultimate element of some of these

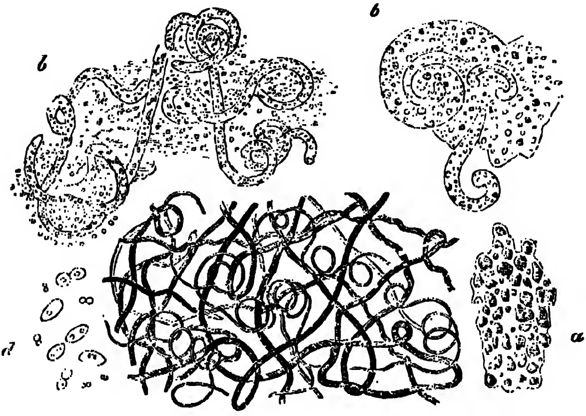


Fig. *e*.

Embryonic Spiral Fibres met with in a solution of Ammonic Carbonate and Sodic Phosphate. (x 600.)

a. Amorphous granules in an almost transparent matrix.
b, *b*. Formation of embryonic fibres by a differentiation of a more finely granular matrix.
c. Similar fibres more fully formed.
d. *Torula*-cells from same solution.

being excessively minute and inextricably twisted. Very peculiar embryonic-looking fibres closely related to them have also been found in an ammonic carbonate solution [1].

[1] In four or five subsequent attempts to produce similar fibres in carbonate of ammonia solutions, I have met with no success. In the first experiment an unknown quantity of the saline materials was dissolved in some of the West Middlesex water. Even if the quantities

These spiral-fibre masses have been met with on seven[1] separate occasions, and in another experiment small masses having a close general resemblance were found where the experimental fluid consisted of a solution of potash and ammonia alum containing a fragment of cheese. The degree of spiral twisting and the character of the fibre itself varied somewhat in different specimens and even in different parts of the same fibre. Some portions were very fine and gradually attenuated, so that this character, in conjunction with their spiral disposition, gave them a very close resemblance to miniature vine tendrils. Some parts of the fibre seemed solid and not much twisted, whilst in others it widened out into flat expansions: portions directly continuous with them occasionally assumed the appearance of very minute fungus filaments. Here the fibre seemed hollow, though it was neither marked off at intervals by dissepiments, nor did it contain protoplasmic masses or granules in its interior[2]. In one of the solutions of ammonic tartrate three distinct masses of the tangled fibre were found, and intertwined amongst the branches of one of them there was an undoubted mycelial mass. This was made up of very delicate filaments, varying much in size even at short distances—not distinctly dissepimented, but showing constrictions at intervals. These filaments were not hollow, but seemed to be filled with an almost homogeneous and very minutely granular protoplasm mass. The wall of the filament

had been accurately known, however, no more success might have attended my efforts to reproduce these fibres than has been met with on many occasions when I have sought to reproduce *Sarcina*.

[1] Five times in solutions containing ammonic tartrate and sodic phosphate, once where the tartrate was replaced by ammonic carbonate, and once in a solution of sodic silicate and ammonic phosphate.

[2] In this condition it bore a very close resemblance indeed to an undoubted mycelial growth often obtainable from the old, brownish pellicle which forms on a fluid in which there are some decaying water plants (*Potamogeton*) or algæ. The filaments of these were equally delicate and devoid of internal granules or dissepiments.

had no appreciable thickness, and appeared to be only the slightly condensed outer layer of the protoplasm of which it was composed. Here and there it appeared that the spiral filament gradually merged into that presenting the more ordinary mycelial character. On this subject, however, I unfortunately cannot speak with absolute certainty, because of the difficulty experienced in accurately tracing any one portion of the filament. On two occasions, moreover, sporangium-like bodies have been found emerging from amidst the filaments, and apparently in connection with them. The nature of these bodies, also, must still be considered as very doubtful. They may be mere plate-like expansions

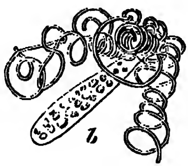


Fig. *d*.

Sporangium-like body amidst mass of Spiral Fibre. (x 600.)

similar to those represented in Figs. *a* and *e*. But nevertheless, their existence, the alterations in appearance of the fibres here and there, and the apparent continuity of these with undoubted mycelial filaments, makes it still impossible to come to a final and satisfactory decision as to the real nature of the spiral fibres[1].

Another subject now claims our consideration. Living matter being the result of a chemical combination of a

[1] In connection with this subject the observations of M. Trécul ('Comptes Rendus,' t. lxi. p. 435) must not be forgotten. He seems to have actually observed the more or less immediate passage of a statical into a dynamical aggregate (*Amylobacter*). The statements and observations of such an observer should not be lightly set aside or ignored.

certain kind, there is no absolute improbability in the supposition that the carbon usually existing in the living compound might be replaced by some other element. With the hope of throwing some little light upon this very difficult subject, I made several tentative experiments with saline solutions containing—in addition to nitrogen, oxygen, and hydrogen—some other element in the place of carbon. The element with which the carbon was replaced was either silicon, boron, chromium, aluminium, or iron[1]. Except in those in which carbon was replaced by silicon, no living things have been met with in any of these solutions (after they had been boiled and the necks of the flasks had been sealed during ebullition). This result—taking it merely for what it is worth—is extremely interesting and suggestive, since silicon is certainly the element which most closely resembles carbon, and which might therefore best replace it in compounds otherwise similar to those which constitute the basis of living matter. A silicon alcohol and ether has in fact been produced by Professor Wöhler[2], in which the carbon of the ordinary compounds is replaced by silicon. It is therefore deemed quite possible that silicon may take the place of carbon in certain forms of living matter. No absolute proof of this, however, can at present be advanced. What follows must be taken merely as an indication of the possibility of such an occurrence.

In the first place (though it is a fact which I have only quite recently observed), minute fungoid organisms have been found growing at the surface of a solution of silicate of soda in a tolerably luxuriant manner. About half an ounce of this fluid was contained in a corked 1 oz. phial, which had not

[1] As I have already stated, these experiments were merely tentative. It is not supposed that solutions were employed free from all trace of carbon, existing as an impurity.

[2] 'Ann. Ch. Pharm.' cxxvii., and civ.

been opened or disturbed for about six months. When accidentally observed a short time ago, it was found to exhibit a flake-like cloudy mass near the surface of the fluid, which I at first supposed to be some insoluble modification of silica. On taking a portion of it out, and submitting it to examination by a high power of the microscope, it was observed that the cloudy mass was wholly composed of the densely interlaced filaments of a fungus mycelium, some of the branches seeming to originate from a large brownish-yellow sporangium, which gave issue to multitudes of filaments on all sides, whilst others bore tufts of spores. Concerning the mode of origin of this fungus nothing can be said—it may, of course, have originated from a spore which had gained access to the solution. The conditions of its nutrition and growth do, however, present features of considerable interest. It was growing quite close to the surface, and may therefore have obtained its nitrogen either from the air or from that dissolved in the surface-layers of the fluid. Did it, however, contain carbon (from some impurity in the form of a carbonate), or was this replaced in the structure of the fungus by silicon from the silicate? This is a question which cannot at present be answered. At all events, the fungus thrived in this solution, and seemed to grow much as it would have done in a solution of ammonic tartrate [1].

In an experiment in which about ten minims of the weak solution of iron pernitrate and seven of sodic silicate solution were added to an ounce of distilled water, the fluid was boiled for fifteen minutes, and the neck of the flask was then hermetically sealed during ebullition. Some semi-gelatinous, reddish-yellow flakes were deposited during the ebullition. The vacuum being still well preserved, the flask was opened

[1] This observation naturally recalls those made by Messrs. Roberts and Slack concerning the growth of fungi on freshly-prepared colloidal silica. ('Quarterly Journ. of Microsc. Science,' 1868, pp. 105–108.)

on the 35th day, when the reaction of the fluid was also found to be still slightly acid. On one of the above-mentioned flakes there was found a minute whitish mass about the size of a small pin's head, which, on examination, was seen to consist of a mycelial tuft, having small but perfect filaments, though without any trace of fructification. The filaments themselves were about $\frac{1}{15000}''$ in diameter, but varied slightly in size, and contained a minutely granular protoplasm without dissepiments. The numerous branches came off at right angles, and the whole organism had all the appearance of being a living fungus.

The other silicate solutions in which organisms have been encountered were quite different in composition. They have been prepared by adding to one ounce of distilled water three grains of ammonic phosphate and about eight minims of sodic silicate solution. Such a mixture always had a slightly alkaline reaction, and it was sometimes used in this condition and sometimes after it had been rendered neutral or very slightly acid by the addition of a few drops of dilute phosphoric acid. The addition of the phosphoric acid seemed, however, to modify the result very much, since four slightly alkaline solutions with which experiments have been made have proved entirely barren, whilst three out of five solutions whose alkalinity had been neutralised by the acid, either contained organisms or spiral fibre masses. In all cases the solutions were boiled[1] for from three to five minutes, and the necks of the flasks were hermetically sealed during this process, and after the expulsion of all air. One flask, which had been prepared six months previously, and whose vacuum was ascertained to be scarcely if at all impaired, was found, when opened, to contain a fluid which still had a very

[1] The silicates are held in solution very feebly, and, unfortunately, are in part precipitated, by the process of ebullition, in the form of bluish-white, cloud-like flakes, which show, on examination with high powers of the microscope, a very minutely granular composition.

slightly acid reaction. The numerous bluish-white flakes which it contained presented almost the same appearance as at first. Amongst these was found a minute whitish mass, about a line in diameter, made up of very delicate mycelial filaments, partly twisted around a cotton fibre. Near the centre of the mass was a large, brown, flagon-like body, about $\frac{1}{235}''$ in diameter, from all parts of the surface of which issued the mycelial filaments whose ramifications went to constitute the rest of the mass. One or two smaller growths were also found attached to some of the flakes, as well as several distinct spore-like bodies of different sizes—mostly of a brownish colour, and having thick walls with granular contents. A group of fine spore-like bodies was also found; these being larger ($\frac{1}{600}''$ in diameter) and colourless, rather than brown. Their nature was altogether uncertain. A solution with an alkaline reaction which had been prepared at the same time, and opened after a similar interval, revealed no trace of spore-like bodies, or of organisms; and two other solutions containing sodic silicate and ammonic bichromate, whose periods of preparation and examination were also similar, were similarly unproductive. All four solutions had been prepared with distilled water taken from the same bottle.

In another of the boiled silicate solutions which had been rendered neutral by the addition of a little dilute phosphoric acid, and which was opened after six weeks (the vacuum being still preserved), two small white masses about $\frac{1}{15}''$ in diameter were found amongst the bluish-white flakes, which, on microscopical examination, proved to be masses of tangled and spirally-coiled fibre, having a very close resemblance to those which had been found in the ammonic tartrate solutions. As was the case with some of these, also, there were, on certain portions of this fibre, plate-like expansions, some of which were irregular in outline whilst

others were quadrilateral bodies, such as I have found on other occasions existing separately (Fig. *a*), and which I believe to be closely related to *Sarcina*. On the surface of

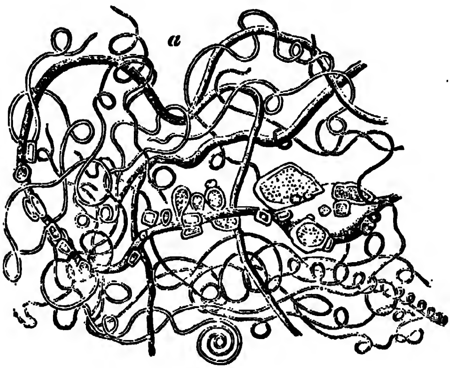


Fig. *e*.

Spiral Fibre with Sarcina-like bodies attached, from a solution of Sodic Silicate and Ammonic Phosphate. (x 600.)

several of the granular flakes there were found very small portions of a similar spiral fibre, which might have represented the nuclei or starting-points of other future masses.

The resemblance of these spiral fibre masses, and of the *Sarcina*-like bodies and fungi found in the siliceous solutions, to those which have been met with in the ammonic tartrate solutions is very striking, more especially when taken in conjunction with the fact that neither spiral fibre masses, *Sarcina*-like bodies, nor organisms, have been met with in the other saline solutions (with which experiment has been made), in which carbon was ostensibly replaced by some other element. It is, moreover, a fact of much significance that no trace of anything like spiral fibres or *Sarcina* has been found in more than one hundred and fifty other flasks similarly prepared with organic infusions of various kinds.

APPENDIX B.

On the living Matter and Organisms contained within Crystals of Neutral Ammonic Tartrate.

ALTHOUGH 'germs' are supposed by many to be universally diffused, more especially in the air and within organic substances, it seems only reasonable for all to suppose that they would exist much less abundantly in saline materials than within organic substances. In order to ascertain whether any, or what, *visible* organisms or spores were to be found in the saline materials employed in my experiments, portions of them have been repeatedly dissolved by distilled water in a watch-glass, and the fluid has afterwards been submitted to the most careful microscopical examination. Moreover, after sufficient time has been allowed for subsidence, the bottom of the watch-glass has been most carefully scrutinized by a powerful immersion lens. The saline materials employed in these experiments have been potash-and-ammonia-alum, tartar emetic, neutral sodic phosphate, neutral ammonic phosphate, ammonic oxalate, ammonic acetate, ammonic carbonate, and neutral ammonic tartrate. The result of repeated examinations of these substances in the manner above stated has been, that not a trace of anything like an organism—no fungus-spore, germ, or egg of any kind—has been found in solutions of any of the substances employed, except in one. The one in which such bodies have been

found is that which I have named last—the neutral ammonic tartrate.

Several of these salts—the oxalate, the acetate, the carbonate, and the tartrate of ammonia—contain within themselves all the elements necessary for the building up of organic substances. Nitrogen, carbon, hydrogen, and oxygen are there, and they only require to fall or to be brought into other modes of collocation in order to give birth to an organizable compound. The crystals of the oxalate are very small, those of the acetate are very deliquescent, and carbonate of ammonia exists generally in the form of non-crystalline cakes[1]. The neutral tartrate, however, exists in the form of tolerably large prismatic crystals, and it was within these only that living matter and organisms were found.

Before describing these organisms more particularly, it will be well to glance for a moment at the origin or mode of preparation of ammonic tartrate. The tartaric acid entering into its composition is obtained from *argol*, the crude potassic bitartrate derived from the grape. And although this latter salt is derived from the tissues of a living plant, the processes to which it is submitted, in order to obtain the tartaric acid in an uncombined state, would most certainly suffice to destroy all living 'germs' which it might have contained. After a solution of the potassic bitartrate has been boiled for a time, calcic tartrate is gradually precipitated by the addition of chalk and calcic chloride. The insoluble calcic tartrate, after having been washed several times, is then brought into contact with *strong sulphuric acid, diluted with only about four times its bulk of water*, and this mixture is boiled for half an hour[2]. All this is necessary before a filtrate

[1] Obtained by a process of sublimation at high temperatures.

[2] The boiling point of such a solution would be several degrees above 100° C. Heat and acid combined, exercise a most powerfully destructive influence upon organic matter, though even *very* dilute sulphuric acid,

can be obtained from which the first crystals of tartaric acid are procurable. Ammonia, the other constituent of the neutral tartrate, being a product of the destructive distillation of coal tar—and itself exercising such a destructive influence upon organic matter when existing in the form of strong *liquor ammoniæ*—would not seem to be a very promising nidus for living 'germs.' The neutral tartrate of ammonia is, however, prepared by mixing a solution of tartaric acid, procured as above mentioned, with an adequate quantity of liquor ammoniæ, and then evaporating the mixture at a gentle heat. Thus prepared, the crystals contain a notable quantity of water of crystallization.

In the stock of crystals procured from Messrs. Hopkin and Williams[1], which had been made about six months previously, some were well formed, and almost perfectly transparent, whilst others were less regular in shape, and presented an opaque appearance with more or less of striation within. When a crystal of moderate size was taken, about $\frac{1}{3}''$ in diameter, or a portion of a larger one, and placed in a large watch-glass with some distilled water, it was frequently found that at first a certain number of opaque-white scales, having a granular aspect under a high magnifying power, dropped from the surface of the crystal to the bottom of the watch-glass. This material, which seemed to have been produced by some superficial alteration (efflorescence) of the substance of the salt, dissolved with much more difficulty than the unaltered matter of the crystal. It remained for a long time at the bottom of the glass, and only very slowly disappeared. As the substance of the crystal thus dissolved away, a number of large and small gaseous bubbles gradually escaped from it. When the crystal was examined

at *ordinary* temperatures, has been found to be peculiarly destructive to all living things.

[1] Of New Cavendish Street.

with a one-inch object-glass whilst solution was taking place, these air bubbles could be seen at first within cavities, from which they were afterwards liberated by a solution of their walls. Occasionally, from the very centre of a crystal from which bubbles of gas had been escaping, a very small and almost invisible filamentary mass floated out, which was more or less thickly studded with minute air bubbles. Such masses were just visible with an ordinary pocket-lens, and when transferred on the point of a needle to a slip of glass, and examined with a magnifying power of about 600 diameters, they were found to contain more or less of the following constituents:—1. A minute fragment of cotton or paper fibre; 2. A variable quantity of an almost transparent, insoluble plate-like substance, homogeneous, though broken up in all directions by intersecting cracks; 3. More rarely, a small quantity of a tenacious mucoid matter containing refractive protein-looking granules of various sizes; 4. A quantity of a colourless, confervoid-looking mass, some of whose smaller filaments, $\frac{1}{20000}''$ in diameter, looked like a mere linear aggregation of irregular masses of protoplasm, though it became obvious that in certain larger filaments, continuous with these, the irregular protoplasm masses were

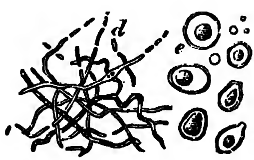


Fig. *f*.

Spores and Filaments similar to those found within Crystals of Ammonic Tartrate. (×600.)

contained within a delicate hyaline cylinder, across which dissepiments were sometimes to be seen, as in very minute

fungus-filaments; 5. And lastly, certain fungus-spores in almost all respects similar to those which have been met with in several of the saline experimental fluids. Although four or five of these were frequently interspersed amongst the confervoid-looking filaments, they did not seem to be in organic connection with them.

Repeated examination of crystals during their solution convinced me that such organic bodies invariably came from the interior of the crystal, often from its very centre, and that they were not to be met with on its surface. Seeing, however, that minute shreds of cotton or paper fibre also came as frequently from the interior of the crystal[1], it was obviously possible that the organisms met with might have been engaged mechanically during the process of crystallization, just as it must have happened with the shreds above mentioned. From what has previously been stated concerning the mode of preparation of the neutral tartrate of ammonia and the origin of its constituents, it may be considered almost certain that these organisms could not have pre-existed in the strong *liquor ammoniæ*, and that all living organisms which might by chance have been associated with the potassic bitartrate must have been hopelessly destroyed by the boiling with sulphuric acid, which occurred at one stage in the process employed for the separation of the tartaric acid from its base.

It is, of course, possible that certain spores existing in the adjacent atmosphere might have dropped into the fluid during the subsequent process of crystallization of the tartaric acid from its mother-liquor, and that these spores might after-

[1] I had often been surprised at finding such shreds when I submitted some of my experimental fluids to microscopical examination, knowing that I had frequently used freshly prepared distilled water, and had taken every precaution thoroughly to cleanse the flasks which were employed.

wards have become mechanically enclosed within the crystals. A similar chance of contamination by spores derived (either mediately or immediately) from the air would exist during the process of crystallization of the ammonic tartrate itself.

If the air, however, had been the immediate source of the fungus-spores and masses of confervoid-looking filaments, then such bodies *might* be found in freshly-prepared crystals just as well as in those which had existed for six months. I therefore asked Messrs. Hopkin and Williams to prepare for me a fresh batch of crystals of neutral ammonic tartrate. This they were kind enough to do; obtaining them in the same place, by the same process, and exposing the mother-liquor in a precisely similar manner.

An examination of some of these crystals, whilst they were being dissolved in a watch-glass by distilled water, showed that (unlike the older crystals) they were not at all coated on the surface by the comparatively insoluble granular plates, and that only a few very small air bubbles emerged from their interior. No trace of the confervoid-looking filaments or of the fungus-spores was to be seen at the bottom of the watch-glass, either during solution or afterwards, though minute shreds of cotton and paper fibres were met with, similar to those which had been found in the older crystals. The examination of a large number of the new crystals was attended with results similar to those just mentioned.

This absence of the *confervoid-looking filaments* and of the *large fungus-spores* from the recently prepared crystals must be accounted for either by one or other of two suppositions:—

(1). It may be supposed that in the case of the older crystals, the spores and filaments had dropped, as such, into the solutions in which the tartaric acid alône, or the tartrate of ammonia, was crystallizing; that they were mechanically engaged in the crystals and were subsequently liberated un-

changed (without having undergone any growth or development) on the solution of the crystal[1]. Whilst, on the other hand, in the case of the recent crystals, it may have happened that no such filaments or spores were floating in the atmosphere, or were present in the water, at the time of *their* formation, so that none could have dropped into the solutions, or could have been enclosed within the crystals.

This is, I think, unlikely to be the real explanation of the difference between the two sets of crystals, and my reasons for this opinion will appear more fully during the discussion of the alternative supposition.

(2). It may be supposed, on the other hand, that the confervoid-looking *filaments* and the *spores* are living units which have assumed their existing forms and dimensions by a process of growth and development within the crystal, and that *the starting-point of each was a mere speck of living matter.*

By adopting this supposition, the panspermatists derive the full benefit of our microscopical researches, and thus narrow their real requirements in the matter of pre-existing spores. It becomes a much simpler case for them, if instead of being compelled to calculate upon the pre-existence of fully formed fungus-spores, and of confervoid-looking filaments, they need only presume upon the pre-existence of a mere speck of living matter less than $\frac{1}{50000}''$ in diameter. I most candidly confess, however, that the pre-existence of such specks of living matter is all that is really necessary[2].

[1] If they had been engaged within the crystals of tartaric acid, they must have been liberated from them during the preparation of the neutral tartrate, only to be re-entangled whilst the crystals of this salt were forming.

[2] Although this supposition is so far favourable to the views of the panspermatists, since it makes their real requirements much more simple, they will find it a most troublesome and unpliant supposition, unless they are disposed at the same time to become out-and-out developmentalists. Their position would, doubtless, be a much more easy one than it is at present, if they chose to maintain that such specks of living

Most of those who have worked much at the microscopic investigation of the organisms met with in organic infusions, must have come to the conclusion that there is no break in the continuity of that developmental series which commences with the mere speck of living matter—the primordial plastide particle—and thence proceeds through such forms as the *Bacterium*, the *Vibrio*, the *Leptothrix* filament, and the mycelial filament of a microscopic fungus. Not that this is an absolutely necessary order of development, or one which invariably occurs—far from it. But as some *Bacteria* commence their visible existence in the form of plastide particles, so some *Vibriones* are but the developed representatives of certain *Bacteria*. And similarly the various kinds of *Leptothrix* filaments grow from certain pre-existing *Vibriones*, just as certain of these *Leptothrix* filaments themselves may perchance become modified into larger segmented fungus-filaments, which, under favourable conditions, may fructify and produce spores—each of which is capable of developing into an organism, like the parent in its latest phase of evolution. Originating, then, in the form of the minutest visible speck of living matter, it seems almost certain that

matter—whatever their precise origin may have been—are practically mere specks of indifferent living matter, having no inherent tendencies, but plastic to the full, and capable of growing into such forms as their environing conditions may determine. And, unless the panspermatists do adopt some such extreme developmental views as these, they will gain comparatively little from the concessions which microscopical investigation compels us to make to them. They will be better able to reconcile their position with the comparative paucity of definite spores and germs which are actually detectable in the atmosphere; but, if they wish to retain their old notions concerning the distinct and uninterchangeable nature of organic species, they will find it as difficult as ever to account for the fact *that the right spores or germs should always be in the right place at the right time*. Very little short of a belief that each cubic inch of air contains the germs of myriads of organisms which are known, or which may hereafter be found under previously unknown sets of conditions, would be adequate to account for all the observed and observable correspondences between the organisms found and the precise nature of the fluids employed.

an organism may pass, more or less rapidly, through the *Bacterium* and the *Vibrio* phase and grow into a *Leptothrix* thread, which, in its turn—by further growth and development—may give rise to a microscopic fungus producing large and definite 'spores.' These fungus-spores, under similar influences, are capable of developing at once into a mycelium similar to that from which they have been produced. They do not again go through the lower terms of the series, but are veritable spores, serving only immediately to reproduce a fungus. On the other hand, it is an undoubted fact, which, although often stated, is not generally known or admitted, that *Torula*-cells and other fungus-germs may also originate as minutest visible specks of living matter which, instead of passing through the stages of *Bacterium*, *Vibrio*, *Leptothrix*, grow and develop at once into fungus-spores, previous to the formation of a fungus-mycelium.

There is, indeed, strong reason for believing that the spores and confervoid-looking filaments in question have not dropped as such from the atmosphere, but that they are, rather, living units which have developed within the crystals. It is almost impossible not to be struck with the improbability of the former of these alternatives, when we take into account the number of such large spores and filaments which, by this supposition, would require to have been present in the atmosphere over the crystallizing materials, as compared with the extremely limited number of such large organisms, which have ever been obtainable when experimental observations have been made upon the nature of the solid particles existing in the air of all ordinary localities[1]. The best evidence in proof of the view that they are products of a development which has taken place within the crystal would be obtained, if it could be shown that in a given

[1] In all my investigations I have never met with spores exactly *similar* to these, except in one or other of the ammoniacal solutions.

batch of recently prepared crystals no such organisms were to be found, whilst in many other crystals belonging to the same batch, after an interval of weeks or months, the spores and filaments were to be discovered. Certain of the crystals of the batch prepared for me by Messrs. Hopkin and Williams, when examined two days after preparation, were found to contain scarcely a trace of air within. But after an interval of three weeks, through which they had been kept during the day-time at a temperature of about 80° Fahr., certain other of these crystals, when dissolved, gave exit to a notable quantity of air bubbles. Two weeks afterwards three other specimens from the recent batch of crystals were examined. The quantity of gaseous bubbles which escaped from them seemed almost equal to those which had been set free from the older crystals. One or two small fragments of cotton also emerged, and in addition several very small masses of a transparent mucoid material, containing refractive protein-looking granules of various sizes and shapes. These were almost precisely similar to masses which had been met with in the older crystal. Here and there an early stage (short portion) of a filament was seen amongst the granules, though none of them were sufficiently long to make me certain as to their nature and affinities. Although nothing else was found, the increased quantity of gas and the occurrence of the very small masses of mucoid material seemed to represent a stage in advance of that which was met with at the last examination. They seemed to show pretty clearly that a change of some kind had been taking place in the material of the crystal, which had led to the liberation of some of its constituents in a gaseous condition, and also, perhaps, to a liberation of some of its water of crystallization. Whilst this had been taking place, its other elements may have been grouping themselves anew, and giving rise to fresh products.

I have lately, through the kindness of Mr. Martindale, of

University College Hospital, had the opportunity of examining some old crystals of ammonic tartrate (also prepared by the same chemists) which must have been in the hospital-dispensary for at least ten years. They were contained in a small corked bottle, and many of them were slightly discoloured—having a somewhat dirty aspect. On solution in a watch-glass most of these crystals yielded patches of fungus-filaments and spores, bearing a very close resemblance to those which had been previously seen. The quantity, however, was far larger than that met with in the more recent crystals, and here the growths existed on the surface of some of the crystals as well as in their interior. In these patches, which had apparently grown through the crystal, the filaments were also more developed.

All the evidence, therefore, tends to show that growth takes place within the crystal, and that the quantity of the filaments and spores increases with the age of the saline matter.

Supposing, however, that the spores and filaments have grown within the crystal, and that they are the developed representatives of certain specks of living matter, two views may still be taken as to the origin of such specks. Either (1) they are some of the pre-existing 'germs' of the panspermatists, which have become mechanically enclosed within the crystal, or (2) these specks of living matter have been evolved therein by virtue of certain changes and re-arrangements which have taken place amongst the not-living constituents of the crystalline matter and the dead organic particles which it encloses.

Of these two alternative views I am, after reflection on the following considerations and evidence, inclined to believe that the latter is most probably the true one :—

(*a*). It must be remembered that however strange and unlikely a situation the interior of a crystal may appear for the

evolution of organisms, there is every reason for believing that cavities occur or are formed within crystals of ammonic tartrate[1], and also (as I have just attempted to show) that the confervoid-looking filaments and the fungus-spores have undergone a process of *growth* and *development* within such cavities. But if 'the conditions' are favourable enough to permit, or even to stimulate, the molecular activity of certain living particles; and if such molecular activity, whereby the living specks grow and develop, is but the modified manifestation of incident physical forces, I see no theoretical reason why the self-same physical forces acting upon the self-same materials should not have been able, in the same place, to *initiate* a molecular collocation similar to that which they now help to build up from moment to moment. We have been, perhaps, only too much in the habit of looking upon this as impossible. But ignoring, as far as we can, this habit of mind for the moment, let us look at the facts as they are. Will it be at all easier for those who believe in no special 'vital principle,' to understand how from moment to moment not-living matter is converted into matter which lives? This process is continually taking place in all growing representatives of the vegetable kingdom, but no one ever thinks of doubting its occurrence merely because he is unable to understand *how* it takes place. If it is conceded that a *de novo* evolution of specks of living matter is possible, then, I think, most physiologists will at once admit, that where specks of living matter are able to grow

[1] The gases which appear in bubbles increase in quantity with the age of the crystal, and they have been seen lodging in cavities within the crystal. These cavities are, perhaps, more especially liable to form in those crystals which are not perfect in shape, and which present a more or less opaque appearance in their interior. Such less perfect types are possibly, on account of this defective form, more prone to undergo molecular changes under the influence of incident forces, especially in the neighbourhood of and around some fibre-fragment which has been enclosed.

and develop, there also they *may* be quite capable of originating.

(*b*). The matter of the crystals of ammonic tartrate is, by a re-arrangement of its atoms, quite capable of giving origin to organizable compounds, and seems, moreover, to lapse into these new modes of combination, with especial facility—a facility far superior to that which is displayed by many other ammoniacal salts[1]. If a small quantity of tartrate of ammonia is dissolved in a watch-glass with distilled water, and is protected as much as possible from dust and evaporation by being covered with two or three inverted glasses, it will be found, during warm weather, that in the course of two or three days the bottom of the watch-glass is covered by a number of minute microscopic crystals, interspersed amongst a mixed layer composed of plastide particles, *Bacteria*, and minute *Torula* cells[2]. These organisms form, in fact, almost as freely (though not so quickly) in this ammoniacal solution, as they do in an ordinary infusion containing organic matter. There can be little doubt that the amount of ammonia and of tartaric acid actually diminishes, and that the elements of these enter more or less directly into the new combinations of which living matter is composed[3].

(*c*). It may be said that such changes do not take place by the mere action of physical forces upon the organic fragments and the molecules of the dissolved tartrate of ammonia, and that the presence of pre-existing living matter is necessary for the initiation of such molecular re-arrangements. In

[1] See *Appendix* C. pp. xlvi–l.

[2] In saline solutions I have generally seen the organisms first, and have found them accumulate principally at the *bottom* of the watch-glass or other vessel in which the solution may have been contained.

[3] Saline solutions in which spores of fungi were placed, having been analysed previously by M. Pasteur, were again analysed by him after the plants had grown for a time. The proportion of ammonia and of other ingredients was found to have undergone a diminution correlative with the growth of the plants.

answer to this, I can only call attention to the fact, that changes of this kind must have taken place 'spontaneously' in the fluids within the experimental tubes which, after having been submitted to temperatures varying from 133°–153° C. for variable periods, were nevertheless subsequently found to contain living organisms. We are compelled to come to this conclusion, not only because there is not one tittle of evidence at present existing to show that any living thing could live through such an exposure, but because there are very strong reasons indeed which should suffice to convince us, that no living thing could be subjected to such a temperature without its life being certainly destroyed. Therefore, in these cases, the particular molecular re-arrangements must have been initiated without the intervention of living ferments, and they are thus comparable with those that are known to take place in a solution of cyanate of ammonia. Here 'spontaneously,' or with the aid of a little heat only, a molecular re-arrangement occurs, and the saline cyanate of ammonia is replaced by a totally different, though isomeric compound, urea. In order to effect this transformation, no living agency is necessary—none has even been supposed to exist; and there is no more really cogent reason why we should imagine such an agency to be necessary, in order that tartrate of ammonia may undergo a more or less similar isomeric transformation.

(*d*). We find, moreover, different kinds of living things associated with different sets of conditions. In none of the crystals of tartrate of ammonia have I ever found a single distinct *Bacterium*, and there has been the same complete absence of organisms of this kind in all my experimental fluids containing ammonic tartrate and sodic phosphate, which have been sealed in airless flasks. This agreement is very striking, seeing that whenever a similar fluid, or a solution of tartrate of ammonia alone, is exposed to the air, *Bacteria* appear in abundance. There is a marked accord-

ance then between the organisms which would appear to have been produced *de novo* within the previously-heated experimental tubes, and those which come from the cavities within the crystals. The conditions are unfavourable in both cases, and the products which result seem to be very slowly evolved.

Because, therefore, of the close resemblance which must obtain between the mode of formation of the first and of subsequent particles of one of the simplest organisms; because tartrate of ammonia seems especially and peculiarly prone to lapse into living modes of combination; because, in addition to the evidence with respect to this particular change, isomeric re-arrangements of other complex substances are undoubtedly capable of taking place 'spontaneously' without the agency of pre-existing living matter; and because the organisms found in the crystals are actually similar to those which form *de novo* in the experimental flasks,—for all these reasons combined, I deem it to be more probable that the filaments and spore-like bodies found within the crystals of ammonic tartrate, have been slowly developed from specks of newly-evolved living matter, than that they have had any other mode of origin. If it had not been proved that living matter could form *de novo*, there would not be sufficient reason for believing that it had occurred in this particular case; so that those who are still unconvinced upon the general question will, of course, not be much influenced by the evidence now adduced.

APPENDIX C.

Comparative Experiments.

In the following experiments, each fluid (unless a statement is made to the contrary) was *boiled continuously for ten minutes*, after having been placed in its flask. Then, with the neck either open, scaled, or plugged, the bulb of the flask was immersed in a water-bath maintained at a temperature of 80°–95° Fahr., during both day and night [1].

First Set of Experiments (I—XV).

a. Fluid exposed to Air in a Flask with a short Open Neck.

No. I. **Urine** in twenty-four hours was still clear and free from deposit. In forty-four hours the fluid was very slightly turbid, and on microscopical examination *Bacteria* and *Torulæ* were found, though not in very great abundance. In sixty-eight hours the fluid was decidedly turbid.

No. II. **Hay Infusion** in twenty-four hours was still clear. In forty-four hours the fluid was very turbid, and a drop, on examination, showed multitudes of *Bacteria* of different kinds, exhibiting languid movements. In sixty-eight hours the turbidity had become much more marked, and there was also a certain amount of sediment.

[1] When infusions have been employed, they have all been made as strong as possible, and have been filtered before use. *Warm* water has been added in quantity just sufficient to cover the substance which was to be infused (this being cut into very small pieces), an the mixture has then been kept at a temperature of from 110°–130° Fahr. for three or four hours.

No. III. **Turnip Infusion** in twenty-four hours showed a very slight degree of turbidity. A drop, examined microscopically, revealed a number of very minute, but very active, *Bacteria*. In forty-four hours the turbidity had become very well marked.

b. Fluid in contact with Ordinary Air and its Particles; Neck of Flask Sealed after the Fluid had become Cold.

No. IV. **Urine** remained quite bright and clear during the fifteen days in which it was kept under observation in the water-bath[1].

No. V. **Hay Infusion** after forty-four hours showed a well-marked turbidity. In sixty-eight hours there was an increase in the amount of turbidity, and also some sediment. During the next forty-eight hours turbidity and sediment gradually increased, whilst the colour of the fluid (originally that of port wine) became several shades lighter. Except that it grew still lighter in colour, and that the amount of sediment increased, it underwent no further obvious change during the fifteen days in which it remained in the bath[1].

No. VI. **Turnip Infusion** underwent no change during the fifteen days in which it was kept in the bath under observation[1].

c. Fluid in a Flask with a Neck two feet long, and having Eight acute Flexures.

No. VII. **Urine** remained quite bright and clear during the fifteen days in which it was kept under observation in the water-bath[1].

No. VIII. **Hay Infusion** remained bright and clear for twelve days. On the thirteenth day a very slight (almost inappreciable) sediment was seen, which scarcely underwent any obvious increase during the next eight days, though on

[1] Flask still in my possession, unopened.

the two following days (twenty-second and twenty-third) the turbidity became most obvious: much sediment was deposited, and the fluid assumed a much lighter colour[1]. (On the twenty-second day the temperature of the bath was raised to 100° Fahr., for two or three hours.)

No. IX. **Turnip Infusion** remained for four days without undergoing any apparent change. Its neck was then accidentally broken at the fourth joint—a certain amount of fluid still filling the third joint. In this condition the flask was allowed to remain in the water-bath, and the fluid continued quite unchanged in appearance for five days. It was then boiled[2] for three minutes, and the neck of the flask was *hermetically sealed* whilst the fluid was boiling. The flask being re-immersed in a water-bath, the fluid continued quite clear for thirteen days. Its neck was then carefully heated in the spirit-lamp flame till, when red hot, the rapid inbending of the glass showed that the vacuum was still preserved. This being ascertained, the flask was, after a few minutes, replaced in the bath. The next day the temperature of the bath was allowed to go up to 100° F. for three or four hours, and in the evening the fluid was observed to be very slightly turbid. In two days more (*i.e.*, after sixteen days *in vacuo*) the turbidity was well marked, and when the fluid was examined microscopically it was found to contain an abundance of very languid *Bacteria* and *Vibriones*. On opening the flask there was an outrush of very fœtid gas, and the reaction of the fluid was acid[3].

[1] Flask still in my possession, unopened.

[2] The vapour had lost all odour of turnip. Some of the fluid which splashed over was found to be still slightly acid.

[3] This experiment is very interesting in two or three respects. A neck of half the usual length—with only four bendings—sufficed to preserve the fluid for several days; and when this fluid (which had been in the bent-neck apparatus for nine days) was sealed up in the same flask during ebullition, it remained *in vacuo* for thirteen days without under-

d. Fluid in a Flask having a Neck two feet long, bent at right angles shortly above the bulk, and provided with a firm Plug of Cotton-Wool twelve inches in length.

No. X. **Urine** remained quite bright and clear during the fifteen days in which it was kept under observation in the water-bath [1].

No. XI. **Hay Infusion** showed a very slight amount of sediment after forty-four hours, which seemed to increase somewhat during the next three days. The fluid afterwards appeared to undergo no further change, though it remained in the warm water-bath for fifteen days [1].

No. XII. **Turnip Infusion** in four days showed a well-marked turbidity, and also very many flakes of a broken pellicle [1].

e. Fluid (in vacuo) *in a Flask, the Neck of which was hermetically Sealed by means of the Blowpipe Flame during Ebullition.*

No. XIII. **Urine** in forty-four hours showed a very slight amount of sediment. During the next two days the sediment very slightly increased, but was still small in amount. At the expiration of fifteen days, no further increase in the turbidity having taken place, the fluid was examined. The vacuum was still partially preserved, as evidenced by the rapid inbending of a portion of the neck of the flask after it had been carefully made red-hot. When opened, the odour of the fluid was stale, but not fœtid, and its reaction was still faintly acid. On microscopical examination *Bacteria* and *Torulæ* were found in tolerable abundance.

going any apparent change, and then only became turbid under the influence of a higher temperature. Yet some of the same fluid, in a flask which was hermetically sealed during the first ebullition (No. XV.) behaved as such an infusion usually does, and became quite turbid in forty-eight hours.

[1] Flask still in my possession, unopened.

No. XIV. **Hay Infusion** in forty-four hours showed a very slight amount of turbidity. In sixty-eight hours the turbidity was most marked, and there was also a small amount of sediment. In another twenty-four hours it was noticed that the colour of the fluid had become much lighter, whilst the turbidity and sediment had increased. It subsequently continued in much the same state, and the flask was opened on the sixteenth day. The vacuum was found to be almost wholly impaired, whilst the odour of the fluid was sour, and not at all hay-like. On microscopical examination *Bacteria*, *Vibriones*, *Leptothrix*, and *Torulæ* were found in abundance, and the former were very active.

No. XV. **Turnip Infusion** after forty-eight hours showed a well-marked turbidity. In seventy-two hours the turbidity was more marked, and there was a slight amount of sediment. The turbidity also increased during the next twenty-four hours; though, after that, the infusion seemed to undergo no further change. The flask remained in the warm bath for fifteen days, when the fluid was examined. Its odour was not fœtid, but was somewhat like that of baked turnip. *Bacteria* and *Vibriones* existed in abundance, though their movements were extremely languid.

Second Set of Experiments (XVI—XXI).

b. Fluid in contact with Ordinary Air and its Particles; Neck of Flask Sealed after the Fluid had become Cold.

No. XVI. **Simple Turnip Infusion** in twenty-four hours had undergone no apparent change. In thirty-six hours there was slight turbidity, and in forty-eight hours this was most marked and uniform. When the flask was opened, after seventy-two hours, there was an outrush of very fœtid gas; the reaction of the fluid was acid, and, when examined

microscopically, it was found to contain multitudes of very languid *Bacteria.*

No. XVII. **Neutralized Infusion of Turnip + $\frac{1}{2}$ gr. of Cheese**[1], in thirty-six hours showed a well-marked pellicle[2]. When the flask was opened, after seventy-two hours, there was a violent outrush of gas, though the fluid was still neutral. Portions of the thick pellicle were found, on microscopical examination, to be made up of *Bacteria*, *Vibriones*, and an abundance of long, interlaced *Leptothrix* filaments. *Bacteria* also existed abundantly in the fluid, though their movements were very languid.

c. Fluid in a Bent-neck Flask, having Eight acute Flexures.

No. XVIII. **Simple Turnip Infusion** after forty-eight hours showed no change. It was kept in the warm-bath for twelve days, and during the whole of this time the fluid remained quite clear. The tube was then broken $1\frac{1}{2}$ inch above the bulb (which was re-immersed in the bath), *leaving the fluid exposed to the air* through the straight open tube. The fluid at this time was odourless, and its re-action was still faintly acid.

The infusion remained thus exposed for six days without undergoing any apparent change. On the eighth day a very slight whitish sediment was noticed, which had increased in quantity by the tenth day, though there was still no trace of general turbidity. On the eleventh day some of the sediment was examined in a drop of the fluid, and it was found to be wholly composed of rather large *Torulæ* cells—the largest being about $\frac{1}{3000}''$ in diameter, though all the smaller sizes were abundantly represented. Not a single *Bacterium* or

[1] The filtered infusion of turnip was neutralized by liquor potassæ. The cheese (Cheddar) was new and not in the least mouldy.

[2] The fluid itself being somewhat opaque, the first stages of increased turbidity from presence of *Bacteria* could not be detected.

Vibrio could be detected, and the fluid was still quite odourless[1].

No. XIX. **Neutral Turnip Infusion + ½ gr. of Cheese,** showed no perceptible change in twenty-four hours, though in thirty-six hours there was a well-marked pellicle on the surface. When the neck of the flask was broken after seventy-two hours, the fluid was found to be very fœtid, whilst its re-action had become slightly acid. Portions of the pellicle were found to be made up by aggregations of *Bacteria*, *Vibriones*, and an abundance of *Leptothrix* filaments. The *Bacteria* all exhibited very languid movements.

e. Fluid (in vacuo) *in a Flask which had been Sealed during Ebullition.*

No. XX. **Simple Turnip Infusion** in twenty-four hours showed a very slight amount of turbidity; in thirty-six hours this had increased, and in forty-eight hours there were multitudes of curdy flocculi floating in a tolerably clear fluid. The flask was opened after seventy-two hours, when there seemed to be only a very slight inrush of air. The odour of the fluid was somewhat fœtid, and its re-action was acid. There were multitudes of *Bacteria* and *Vibriones*, partly separate and partly aggregated (constituting the flocculi above mentioned). The separate *Bacteria* exhibited only very languid movements.

[1] This again is a most instructive experiment when compared with Nos. XVI. and XX., in which portions of the same infusion were employed. The results in No. IX. would lead us to believe that a vegetable infusion which does not ferment, does, nevertheless, undergo some changes in molecular composition, and this notion seems to derive confirmation from the present experiment. Some of the same solution which has been kept for a time (twelve days) from contact with atmospheric particles, subsequently, even when fully exposed to the air, undergoes no apparent change for six days, and then, instead of becoming filled with *Bacteria*, swarms only with *Torulæ*. Yet the infusion in this condition was perfectly capable of nourishing *Bacteria*, as I subsequently proved by inoculating it. Why then was it not inoculated by the living *Bacteria*, with which the air is thought by some to be teeming?

No. XXI. **Neutral Turnip Infusion + $\frac{1}{2}$ gr. of Cheese,** showed a well-marked pellicle on its surface in twenty-four hours. In thirty-six hours the first pellicle had, in great part, sunk to the bottom of the flask, though its place on the surface was already taken by a new, though thin, scum-like layer. After seventy-two hours, the flask was opened; there was *no fœtid* odour of the fluid, and its reaction was *still neutral.* Examined microscopically the fluid showed an abundance of *Bacteria,* and also of short monilated filaments. There were, however, none of the ordinary kind of *Vibriones,* and no *Leptothrix.* All the *Bacteria* exhibited very languid movements.

THIRD SET OF EXPERIMENTS (XXII—XXX).

a. Fluid exposed to Air in a Flask with a short Open Neck.

No. XXII. **Urine** in twenty-four hours showed no change; though in forty-six hours the turbidity was well marked[1]. Examined microscopically it was found to contain an abundance of *Bacteria.*

b. Fluid in contact with Ordinary Air and its Particles; Neck of Flask Sealed after the Fluid had become Cold.

No. XXIII. **Urine** in eighteen hours showed a distinct pellicle, though there was not much general turbidity. During the next few days the old pellicle fell to the bottom, and a new one formed.

c. Fluid in a Bent-neck Flask, having Eight acute Flexures.

No. XXIV. **Urine** in forty-eight hours showed no change. After twelve days there was still no general turbidity, though there was a slight flocculent deposit of an un-

[1] Some of the same fluid, exposed in a similar flask, without previous boiling, became turbid in eight hours, and lighter in colour; whilst, after twenty hours, the turbidity was extremely well marked.

certain nature. Two days afterwards the flask was broken, when the odour of the fluid was still found to resemble that of fresh urine, and its re-action was acid. The flocculi were made of granular aggregations, in the midst of which were a few bodies closely resembling *Torulæ*, though they were somewhat doubtful in nature. Neither *Bacteria* nor *Vibriones* could be found. The flask, having a short open neck, was then replaced in the warm-bath. In sixteen hours the whole fluid had become turbid; it was also slightly fœtid; and, on microscopical examination, it was found to be swarming with *Bacteria*, *Vibriones*, and *Leptothrix*.

No. XXV. **Turnip Infusion + $\frac{1}{3}$ gr. of Cheese,** in forty-eight hours showed no change, though in seventy-two hours there was a well-marked pellicle, in which some bubbles of gas were engaged. After ninety-six hours the neck of the flask was broken; the fluid was found to be fœtid, and it had an acid re-action. On microscopical examination, a portion of the pellicle was seen to consist of multitudes of *Bacteria*, *Vibriones*, and jointed *Leptothrix* filaments.

No. XXVI. **Simple Turnip Infusion** remained clear, and showed no appreciable change for seven days. On the eighth day a slight general turbidity of the fluid was noticed. On the ninth, the turbidity was rather more marked, though there was no trace of a pellicle; the neck of the flask having been broken, the fluid was found to be *odourless* and very faintly acid. On microscopical examination, multitudes of languid *Bacteria* of medium size were found, and also short monilated chains with from two to ten segments. There were no *Vibriones*, *Leptothrix*, or *Torulæ*[1].

e. Fluid (in vacuo) *in a Flask, Sealed during Ebullition.*

No. XXVII. **Healthy Urine** after twenty-four hours

[1] The condition of the fluid, and the nature of its contents, was very similar to that met with in No. XXI.

showed no change. After eleven days there was still no apparent change, though on the thirteenth a slight amount of flocculent sediment was noticed. This deposit increased in amount, very slowly, during the next fortnight; though afterwards the fluid seemed to undergo no further change, and did not become generally turbid[1].

No. XXVIII. **Healthy Urine** ($\frac{1}{3}$) **and Filtered Turnip Infusion** ($\frac{2}{3}$), after forty-eight hours showed a very slight turbidity, which, however, became quite marked in another twenty-four hours.

No. XXIX. **Albuminous Urine** ($\frac{1}{3}$) **and Filtered Turnip Infusion** ($\frac{2}{3}$), after twenty-four hours, showed a slight turbidity, which became much more marked in forty-eight hours; whilst in seventy-two hours there was a considerable deposit at the bottom of the flask.

No. XXX. **Simple Turnip Infusion** showed no change in forty-eight hours, though in seventy-two hours there was well-marked turbidity. The turbidity and sediment continued to increase for several days, and both were most marked on the tenth day, when the flask was opened. There was an outrush of gas, having an extremely fœtid odour. The fluid had an acid re-action, and when examined microscopically, multitudes of *Bacteria*, *Vibriones*, and *Leptothrix* filaments were found—the movements of the *Bacteria* being very languid.

Fourth Set of Experiments (XXXI—XXXVII).

b. Fluid in contact with ordinary Air and its Particles; Flask Sealed after the Fluid had become Cold.

No. XXXI. **Healthy Urine** remained in the warm-bath for twenty-eight days without undergoing the least change.

[1] Still in my possession, unopened. In all probability the flocculi which formed would be found to be similar in their microscopical, as

No. XXXII. **Simple Turnip Infusion** remained in the warm-bath for twenty-eight days without undergoing any appreciable change [1]. On breaking the neck of the flask, the fluid was found to be quite odourless. With its neck quite open, the flask was replaced in the water-bath. During the first forty-eight hours it underwent no apparent change, though at the end of seventy-two hours a slight general turbidity was noticeable, and an examination of a drop of the fluid (still odourless) showed a number of minute but very active *Bacteria* [2].

c. Fluid in a Bent-neck Flask, having Eight acute Flexures.

No. XXXIII. **Simple Turnip Infusion** showed no change after immersion for eight days in the warm bath. After eleven days, the fluid being still clear, the tube was broken just beyond the second bending from the bulb, and then the flask was re-immersed in the bath. After three days' exposure, the fluid being still clear, it was boiled in the flask for one minute, when it was noticed that the steam was quite odourless. The flask was then replaced in the water-bath, where it remained for twenty-two days (still with the neck open and broken just beyond its second bending) without showing any change [3]. It was then submitted to examina-

they certainly were in their naked-eye characters, to those met with in No. XXXV.

[1] Experiment No. 8, recorded in 'Nature,' 1870, No. 36, p. 194, may be compared with this and No. XXXIII.

[2] This experiment should be compared with Nos. XXIII. and XXXIII. It seems to show that if some fermentable fluids can be kept for a time under conditions in which they will not ferment, the constitution of the fluid, instead of remaining the same, undergoes a slow alteration by which it is rendered absolutely less fermentable, even when exposed to the most favouring influences.

[3] After this experiment had been completed, a fresh-filtered infusion of turnip was placed in the same flask (having the neck open just beyond its second bending), and after having been boiled for a few minutes it was immersed in the same water-bath. This fluid became

tion; the fluid was found to be devoid of all odour, it had a slightly bitter taste, and its re-action was very faintly acid. On microscopical examination no living things were found; there were no *Bacteria*, no *Vibriones*, and no *Torulæ*, only some mere granules, a small amount of amorphous matter, and a few fibres[1].

No. XXXIV. **Turnip Infusion Neutralized by Ammonic Carbonate** in forty-eight hours showed a slight turbidity, which slowly increased during the next two days. In two days more the turbidity was very great, and there was also a considerable amount of sediment. The fluid was then examined microscopically, and found to contain myriads of large but very languid *Bacteria*.

e. Fluid (in vacuo) *in a Flask which had been Sealed during Ebullition.*

No. XXXV. **Healthy Urine** underwent no apparent change for the first twelve days, then (the bulk of the fluid still remaining clear and bright) small greyish-white flocculi began to collect at the bottom of the flask, which very slowly increased in quantity during the succeeding twelve days. At the expiration of this time the flocculi were pretty numerous, though the fluid was otherwise bright. The

turbid in thirty-six hours, and was then found to contain multitudes of *Bacteria;* and the characteristic odour of the turnip infusion was still appreciable.

[1] The results of this experiment are most interesting, especially if compared with that which takes place when some of the same fluid is neutralized by ammonic carbonate (No. XXXIV.), or when a similar fluid (as in No. XXX.) is contained in a flask sealed during the ebullition of the fluid, or with what occurred in Nos. XIII. and XXXII.

vacuum was ascertained to be still good, and on breaking the flask, the fluid was found to have a slightly acid reaction, though no appreciable odour. When examined microscopically, the flocculi were seen to be made up for the most part of mere granular aggregations (simple, and not in the form of *Bacteria*). Small *Torula* cells, however, existed in some quantity; also a few necklace-like chains, and a comparatively small number of *Bacteria*, some of which were tolerably active.

No. XXXVI. **Simple Turnip Infusion** after twenty-four hours showed no sign of change, though in thirty-six hours it was slightly turbid. On the fourth day the turbidity was well-marked and general, though there were no flake-like aggregations. When examined microscopically, the fluid was found to contain multitudes of *Bacteria*.

No. XXXVII. **Turnip Infusion**[1] **Neutralized by Ammonic Carbonate** in twenty-four hours was decidedly turbid. In thirty-six hours the turbidity was more marked, and there was a slight sediment. By the end of forty-eight hours both turbidity and sediment had notably increased. On the fourth day, there was a moderately clear fluid, containing an abundance of curdy or flake-like masses, which when the flask was opened, were found to be made up principally of an aggregation of myriads of *Bacteria*.

Fifth Set of Experiments (XXXVIII—XLVII).

Fluids not boiled, but half-filling hermetically Sealed Flasks, containing Ordinary Air.

No. XXXVIII. **Turnip Infusion** in ten hours showed a slight amount of turbidity. After forty-eight hours this was very well-marked: there was a thick pellicle on the surface,

[1] Some of the same as that which was used (unaltered) in last experiment.

and, in addition, a small amount of deposit. On examination, the fluid and the pellicle were found to contain an abundance of *Bacteria*, *Vibriones*, and *Leptothrix* filaments.

No. XXXIX. **Turnip Infusion + $\frac{1}{20}$ of Carbolic Acid,** after eight days showed no appreciable alteration in appearance [1], no trace of pellicle or deposit. When examined microscopically, however, the fluid was found to contain some very minute *Bacteria*, though they were by no means abundant.

No. XL. **Hay Infusion** had become quite turbid in twenty-four hours, and several shades lighter in colour. After forty-eight hours the colour of the infusion was still lighter; there was more turbidity, and some sediment. On microscopical examination, the fluid was found to contain an abundance of *Bacteria*, *Vibriones*, and short *Leptothrix* filaments.

No. XLI. **Hay Infusion + $\frac{1}{20}$ of Carbolic Acid,** showed no apparent change [2] after forty-eight hours, and when examined microscopically it revealed no trace of *Bacteria*, or other organisms. The neck of the flask was then again closed. On the twelfth day the fluid had still undergone no change in appearance, and when examined microscopically it still showed no trace of organisms, though the fluid was—as it had been at the time of the first examination—full of minute, undissolved particles of carbolic acid.

Fluids boiled for five minutes, and half-filling hermetically Sealed Flasks containing Ordinary Air.

No. XLII. **Hay Infusion,** after forty-eight hours, showed no change, and continued to remain quite clear and free

[1] It had been rendered turbid from the first, by the carbolic acid.

[2] The fluid had been rendered paler and turbid from the first, by the addition of the carbolic acid.

from deposit until the twelfth day, when it was examined microscopically. No organisms of any kind could be detected.

No. XLIII. **Hay Infusion + $\frac{1}{20}$ part of Carbolic Acid,** showed no apparent change[1] for the first five days, though, on the sixth day, a slight deposit was noticed at the bottom of the flask. The deposit had increased, and was well marked by the twelfth day, when, on microscopical examination, there were found, amongst the granular flakes of the deposit, *Torulæ* of several varieties of size and shape. Many were spherical, others ovoid, or having an elongated oat-like shape; some were of the ordinary colour, and others were brownish in tint. The variety was most striking. No *Bacteria* were seen, though there were multitudes of active particles which seemed to differ from the minute spherules of undissolved carbolic acid.

Fluids (in vacuo) *boiled for five minutes, and Flasks Sealed during Ebullition.*

No. XLIV. **Turnip Infusion,** in seventy-two hours, showed a slight turbidity, which gradually increased. On the eighth day there was a considerable quantity of flake-like sediment, and some amount of general turbidity. On the thirteenth day the vacuum was found to be still partly preserved. When the flask was opened the fluid was perceived to have a fœtid odour, and an acid re-action; and, on microscopical examination, multitudes of *Bacteria* and *Vibriones* were seen. In the flake-like aggregations (made up almost wholly of these organisms) there were also a number of large thick-walled spores; some already formed, and others in process of formation.

[1] The alteration in colour was less marked than in the similar mixture which had not been boiled, though the turbidity was just as obvious.

No. XLV. **Turnip Infusion + $\frac{1}{20}$ part of Carbolic Acid,** showed no increase of turbidity[1] for the thirteen days during which it was kept under observation. Before the flask was opened it was ascertained that the vacuum was well preserved. The odour of the fluid was unaltered, and on microscopical examination no *Bacteria*, or other living things, were found [2].

No. XLVI. **Hay Infusion,** after forty-eight hours, showed no change, though, in seventy-two hours, there was perceptible a very small amount of a dirty greyish deposit. By the fifth day the deposit had slightly increased, and on the seventh day there was a trace of turbidity in the fluid. It did not undergo much further change, so that, on the twelfth day, the flask was opened. The vacuum was found to have been very slightly impaired; the odour of the fluid was almost natural, and its re-action was slightly acid. On microscopical examination of the deposit, *Bacteria*, *Vibriones*, short *Leptothrix* filaments, and *Torulæ*, were found, though not in very great abundance.

No. XLVII. **Hay Infusion + $\frac{1}{20}$ part of Carbolic Acid,** showed no apparent change for the first four days. On the fifth day there was a small quantity of powder-like sediment, and one dirty greyish-coloured flake. On the seventh day there were more small flakes at the bottom, and a slight general turbidity of the fluid. On the twelfth day, the turbidity and deposit having increased, the flask was opened —after it had been first ascertained that the vacuum had only been slightly impaired. The re-action of the fluid was still strongly acid. On microscopical examination of some of the deposit, there was found, amongst granular flakes and aggregations, a large number of *Torulæ* cells, of most various

[1] This fluid was whitish, and somewhat opaque, from the first.
[2] For other experiments showing a similar sterility, induced by a slight acidification with acetic acid, see 'Nature,' 1870, No. 37, pp. 226, 227.

shapes and sizes; also in the midst of the granule heaps many large, rounded or ovoidal, densely granular nucleated bodies, whose average size was $\frac{1}{1500}''$ in diameter, though there were many of them much larger, and others even less than half this size. Intertwined amongst the granular matter also were a large number of algoid-looking filaments, $\frac{1}{20000}''$ in diameter, containing segmented protoplasmic contents. There were also in the fluid itself a number of medium-sized, unsegmented *Bacteria*, whose movements were somewhat languid[1].

Sixth Set of Experiments (XLVIII—LXVI).

Ammoniacal Solutions, unboiled, and exposed to Ordinary Air in a Corked Bottle[2]. (Temp. 60°–65° F.)

No. XLVIII. **Ammonic Acetate Solution.**—On the tenth day the fluid was still quite clear, and free from sediment.

[1] The results of this experiment and of No. LXIII. are decidedly opposed to the reality of the germ-killing powers with which carbolic acid has been endowed by Professor Lister and others. I, however, had previously found that specimens of *Torulæ* and *Bacteria*, obtained from freshly-opened flasks and then mounted as microscopical specimens in a mixture of glycerine and carbolic acid (in the proportion of 15 : 1), not unfrequently grew and multiplied under such conditions MM. Béchamp and Estor also found that *Bacteria* multiplied in carbolized fluids, and similar facts have been testified to by some Italian observers. But organic fluids differ much from one another, so that the influence of carbolic might well be different upon different fluids. And, accordingly, we find that whilst its addition to, and subsequent boiling with, a hay infusion increases the fermentability of the latter, precisely the opposite effects are produced when the hay is replaced by a turnip infusion. (See No. XLV.) Without wishing to undervalue in the least the system of treatment introduced, and so admirably carried out by Professor Lister, I am strongly inclined to think that he explains his results by theories which are almost wholly incorrect.

[2] All the simple ammoniacal solutions were in the proportion of ten grains of the salt to the fluid ounce of distilled water; and to those which also contained sodic phosphate, three grains of this were added. About half an ounce of each solution was put into a one-ounce wide-mouthed bottle, and then tightly corked.

No. XLIX. **Ammonic Oxalate Solution.**—On the tenth day there was no distinct opalescence of the fluid, but a well-marked whitish flocculent deposit. On microscopical examination no *Bacteria* were found in the fluid, and the deposit was made up of an aggregation of blackish and colourless granules, mixed with a few crystals and a very few *Torulæ* cells—all being held together by a sort of mucoid matrix. In the midst of this matter were found two or three very small, much branched, mycelial tufts of a fungus growth.

No. L. **Ammonic Carbonate Solution.**—On the tenth day the fluid showed a very faint opalescence, with a small amount of deposit, and a partial non-coherent scum on the surface, which, on microscopical examination, was found to be composed partly of amorphous granules, and partly of minute *Bacteria*, mixed with small necklace-like aggregations. The fluid itself contained, in suspension, a few small and sluggish *Bacteria*, with a minute *Torula* cell here and there.

No. LI. **Ammonic Tartrate Solution** after twenty-four hours showed the faintest opalescence of the fluid; in forty-eight hours there was a bluish-white turbidity, and in seventy-two hours the turbidity was well marked. When examined microscopically, the fluid was found to contain multitudes of very active *Bacteria*. On the thirteenth day the turbidity was not so well marked, though there was a very thin pellicle on the surface, and also the dirty-looking crumpled remains of another pellicle at the bottom, which, on examination, was found to be composed of an aggregation of *Bacteria*. The pellicle on the surface was very thin, and composed only of a single layer of *Bacteria*. In the fluid itself many *Bacteria* were seen, of medium size, and mostly sluggish in movement, though a few of them exhibited very active rotatory movements. No *Vibriones*, *Leptothrix*, or *Torulæ* were found.

No. LII. **Ammonic Tartrate and Sodic Phosphate Solution,** after twenty-four hours showed the faintest opalescence; in forty-eight hours there was a bluish-white turbidity, which, in seventy-two hours, had become more marked. When examined microscopically multitudes of *Bacteria* were found whose movements were very sluggish. On the thirteenth day there was a well-marked whitish turbidity, due to *Bacteria* and *Vibriones*, a slight amount of deposit, and a firm pellicle which was found to be composed, almost wholly, of long unjointed *Vibriones* and unsegmented *Leptothrix* filaments, all of which, when separate, exhibited the most distinct eel-like movements, accompanied by an actual progression from place to place.

Ammoniacal Solutions, unboiled, and exposed to Air in a Corked Bottle, after Inoculation with a Drop of Fluid containing living BACTERIA *and* TORULÆ. (Temp. 60°-65° F.)

No. LIII. **Ammonic Acetate Solution,** after twenty-four hours was faintly opalescent, and in forty-eight hours showed a very slight bluish tint. In seventy-two hours it was in the same state, and, on microscopical examination, the fluid showed no distinct *Bacteria* or other living things, though there were a number of very minute particles distributed, singly or in small groups, throughout the fluid. On the thirteenth day there was no change in appearance, except that the sediment had somewhat increased in amount. Still, no *Bacteria* could be found in the fluid or the sediment,—only the above-mentioned particles, and a few somewhat larger, which resembled very minute *Torulæ*. Amongst the sediment, however, there were two or three very small mycelial tufts of a developing fungus.

No. LIV. **Ammonic Oxalate Solution.**—On the eighth day the fluid showed a very faint opalescence, though there was a well-marked, greyish, flocculent deposit, which was

found to be composed of an aggregation of colourless and blackish granules, of a multitude of minute crystalline particles, mostly diamond-shaped, and some rounded or ovoidal, thick-walled, spore-like bodies; amongst which, and enveloped in part by them, were several mycelial tufts of a fungus. A number of minute *Bacteria* were found distributed throughout the fluid, and also a quantity of minute star-like bodies (crystalline), about $\frac{1}{15000}''$ in diameter.

No. LV. **Ammonic Carbonate Solution.**—On the eighth day the fluid showed a very faint opalescence, and a slight deposit, which was found to be composed principally of amorphous granules. Distributed through the fluid were some small and sluggish *Bacteria*, though no other organisms were seen.

No. LVI. **Ammonic Tartrate Solution.**—After twenty-four hours the fluid showed the faintest opalescence, and in forty-eight hours there was a slight bluish-white turbidity. In seventy-two hours the turbidity was well marked, and there was a very thin pellicle on the surface. When examined microscopically the fluid was found to contain multitudes of very active *Bacteria*, and the pellicle was also composed of an aggregation of *Bacteria*. On the thirteenth day the opacity had somewhat increased; there was also a well-marked pellicle, and an obvious deposit. The pellicle was found to be composed of *Bacteria*, and in the fluid there were multitudes of medium-sized *Bacteria* and *Vibriones*, with here and there a small *Torula* cell[1].

[1] On comparing the corresponding experiments of Series XLVIII—LI. with those of Series LIII—LVI. less difference is found than might have been expected by many. The comparison of the numbers of each series with one another, also reveals the interesting fact that the mere presence of N, C, O, and H, is not all that is required even for the growth and nutrition of the lower living things. These elements seem to lapse into the new combinations constituting living matter of various kinds, more easily from certain pre-existing states of combination than from others. Solutions of ammonic tartrate are much more favourable

Ammoniacal Solutions (in vacuo) *in Flasks which were hermetically sealed during Ebullition of their Fluids at a Temperature of 90° F.*[1] (*Subsequently exposed in water-bath to a Temperature of 75°-85° F.*)

No. LVII. **Ammonic Tartrate Solution** after sixty hours showed a slight sediment, with bluish flakes attached to sides of flask. In eighty-four hours there was a general bluish opalescence, and on microscopical examination the fluid was found to contain multitudes of *Bacteria.*

No. LVIII. **Ammonic Tartrate and Sodic Phosphate Solution.**—After sixty hours there was a slight general bluish opalescence. In eighty-four hours the general opalescence was not more marked, but there were many flake-like aggregations in the fluid, which, on microscopical examination, were found to be aggregations of *Bacteria.*

starting points for the new combinations than solutions of ammonic acetate. The comparison of experiment No. LI. with No. LII. is extremely interesting in reference to the dogma that phosphorus is a necessary ingredient in living matter. Solutions of the ammonic tartrate in distilled water have been twice analyzed for me by a skilful chemist without revealing the least trace either of phosphorus or sulphur. This result is very remarkable when compared with the amount of living matter which may so soon appear in such a solution: the number of the organisms and the rapidity of their evolution, being almost equal to that which occurs in a similar solution to which a phosphate has been added. However much, therefore, phosphorus may aid the development of organisms in many fluids, there is still an important difference between *many* and *all*, which, if more frequently borne in mind, would render universal propositions more scarce (see 'Journal of Chemical Society,' March, 1871, pp. 72-74). The truth of the dictum '*Ohne Phosphor gar kein Leben,*' is, I venture to think, far from being proved. If it be accepted because evidence (referring only to particular fluids) which is really insufficient, such a dictum seems to testify to its truth; and if then, the presence of organisms in any fluid is to be taken as evidence of the existence of phosphorus (even though this cannot be otherwise substantiated), the theoretical relationship of phosphorus to Life would come to be very similar to the old views concerning germs and Life.

Mutato nomine de te
Fabula narratur.

[1] The fluids were boiled at the low temperature, with the aid of an air-pump, simply in order to procure a more perfect vacuum in the flasks; these experiments being destined to show whether the

Ammoniacal Solutions boiled (at 212° F.), and exposed to Air in Flasks whose open necks were only loosely covered with Paper Caps: subsequent Inoculation. (Temp. 75°-85° F.)

No. LIX. **Ammonic Tartrate Solution.**—The fluid remained quite clear, and free from all trace of turbidity up to the ninth day, when it was inoculated with some living *Bacteria*. In fifty hours after the inoculation there was a very faint opalescence of the fluid, which, in another twenty-four hours, had become much more marked. On microscopical examination it was found to contain multitudes of *Bacteria*.

No. LX. **Ammonic Tartrate and Sodic Phosphate Solution.**—After four days the fluid was still quite clear. In seven days no trace of general turbidity, though there was a minute dirty-grey aggregation, about $\frac{1}{14}''$ in diameter, at the bottom of the flask. On the sixteenth day the grey aggregation had very slightly increased in size, though the fluid above was still perfectly clear. The grey mass was removed by a small pipette, and, on microscopical examination, it was found to be composed of an aggregation of minute extraneous fibres, mixed with blackish particles and amorphous granular matter. in which were growing many *Torula*-cells in all stages of development, and also a minute mycelium composed of branched *Leptothrix*-like fibres[1]. The clear fluid was then inoculated with some living *Bacteria*, and the bulb of the flask was replaced in the warm-bath. After fifty hours the solution showed a bluish turbidity, which, in thirty-six hours more, had increased to a well-marked whitish

simple (uninoculated) solutions would become turbid *in vacuo*—that is to say, without the oxidizing influences of the air—when they had not been exposed to an amount of heat sufficient to destroy any living or dead ferments which they might contain.

[1] A deposit of this kind is almost invariably found in such solutions after their degree of fermentability has been lowered by previous boiling. Growth takes place very slowly in these cases.

opacity, and when examined, the fluid was found to be swarming with active *Bacteria*.

Solutions of Ammonic Tartrate and Sodic Phosphate were heated, in their respective Flasks, for Fifteen Minutes to the Temperatures mentioned below. The necks of the Flasks were afterwards loosely covered with Paper Caps, whilst the Bulbs were immersed in a Water-Bath kept at a Temperature of 75°-85° F.

No. LXI.	**Solution**	**heated**	**to**	149° F.
No. LXII.	"	"	"	158° F.
No. LXIII.	"	"	"	158° F.
No. LXIV.	"	"	"	167° F.
No. LXV.	"	"	"	167° F.

All these solutions remained quite clear and free from any trace of general turbidity for ten days. Each fluid was then inoculated with some living *Bacteria*, and in the space of thirty-six to seventy-two hours, all had become more or less obviously turbid, and on microscopical examination this turbidity was found in each case to be almost wholly due to the presence of multitudes of *Bacteria*.

APPENDIX D.

On the Variability of the Lower Forms of Living Matter.

Lichens. Within the last few years the notion has been gradually growing up that many of the so-called unicellular Algæ and their allies are but stages in the growth of the gonidia of certain Lichens. This view was originally started by the valuable observations of Itzigsohn [1]. With regard to *Chlorococcus*, Dr. Braxton Hicks says [2] he has thoroughly satisfied himself that such a relationship occurs between this so-called Alga and the Lichen known as *Parmelia parietinus*. After describing the changes that take place in the vegetàtive multiplication of *Chlorococcus*, and comparing them with those occurring in the similar growth from the gonidia of Lichens, he summarizes the results of his observations thus:—'I think, then, from the above remarks, that there can be no doubt but that what has been called "Chlorococcus" is nothing more than the gonidia of some Lichens, which, having been conveyed by the movements of the atmosphere, had been deposited on a favourable surface, where they soon begin to increase by various modes of segmentation which continue for an unlimited period. But under suitable conditions, chiefly drought and warmth, the gonidium throws out from

[1] 'Botan. Zeitung,' Jan. 5, 1855.
[2] 'Journ. of Microsc. Science,' 1860.

its external envelope a small fibre, which adhering and branching, ultimately encases it and forms a "soridium"[1]. At this stage the gonidium may continue also for an indefinite period in a dormant condition, but, circumstances favouring, segmentation of the gonidium goes on within the soridium, while the branches of the fibre penetrate within the divisions, till at last a young thallus is formed[2]. But a check may occur during any of these stages, and yet vitality be prolonged for a period of months and even years.' Frequently the 'Chlorococcus' stage continues for a very long time, and the products resulting from it constitute a green coating on walls, trees, and other external objects, on which it may cover large surfaces. The continuance of this algoid stage seems to be favoured by the occurrence of cool moist weather; and its tendency to persist in this stage, taken in conjunction with the almost universal presence of the gonidia of Lichens[3] in snow and rain water, may help to account for the wide diffusion of this green algoid coating.

Although the *Chlorococcus*-gonidium and the *Lichen*-gonidium usually undergo precisely similar segmentation-changes till soridia appear and the real lichen-thallus is ultimately produced, yet, in other cases, the soridia undergo another remarkable set of changes, instead of passing on directly to the development of a thallus. The changes about to be described have been observed by Dr. Hicks principally

[1] See loc. cit., Pl. x.

[2] Dr. Hicks says:—'It will be easily perceived that the soridium contains all the elements of a thallus in miniature; in fact, a thallus does frequently arise from one alone, yet, generally, the fibres of neighbouring soridia interlace, and thus a thallus is matured more rapidly. This is one of the causes of the variation in appearance so common in many species of Lichens.'

[3] After a series of observations extending over many years, Dr. Hicks is enabled to say, that 'they may be collected in comparatively great numbers from snow and rain, particularly the former.'

in *Cladonia pyxidata*, though similar processes have been watched in connection with the gonidia of other Lichens belonging to the same and to different genera.

When growing in a dry situation (or if the season is hot and dry) the gonidia of *Cladonia pyxidata* behave in the same manner as those already described of *Parmelia* and *Chlorococcus;* but, on the contrary, if the weather becomes damp, if the plant grows in a damp situation, or even if removed to one, then other and quite different changes ensue.

Under any one of such circumstances, Dr. Hicks says:—'The first change observable is that some of the segments become enveloped by a layer of mucus, inside of which subdivision still further proceeds, the portions in most cases possessing, after a little time, each a separate mucous envelope [1]. Thus we have all the elements of a *Gleocapsa* (Kützing) growth. At first, commonly, the subdivision is continued on the binary plan, which may continue for some time.' Other modes of subdivision, however, may follow. The early segments may separate from one another, each provided with a mucous layer, but in others the mucous envelope of the original cell does not dissolve away, though segmentation proceeds in its interior. Thus masses result, in which the Gleocapsiform cells have from one to three common envelopes, and a condition is produced, as Dr. Hicks points out, similar to Hassal's *Hematococcus rupestris*. But, he says:—'In the same mass—the produce of the *Cladonia*-soridia—will be found every variety of subdivision, each form constituting a mass of a greater or smaller extent,' though these various products were not always indiscriminately mingled, 'as if a particular kind having once commenced, it would,

[1] All the changes which are now being described are accurately represented in Pl. ii. of the 'Journ. of Microsc. Science' for 1860.

circumstances continuing the same, proceed in the same direction for an unlimited time.' Dr. Hicks tells us still further that—'When the soridia, undergoing this transformation, are placed in water, the mucous envelope becomes much increased in diameter, the cells become more numerous and smaller, and assume the appearance of *Hematococcus alpestris*. It proceeds sometimes to such extreme division that the process seems almost indefinite, and the results resemble *Hematococcus theriacus*, and *minutissimus*, Hass.' Segmentation goes on, in fact, in various ways, and the proportion that the mucous coat bears to the cell is exceedingly variable. At other times a group of large oval cells is produced, each of them surrounded by a mucous layer, precisely similar to the *Palmoglea* of Kützing (*Coccochloris*, Hassal). The common mucous envelope soon disappears, and then the contained cells rapidly subdivide and form an indefinite mass resembling *C. Brebissonii*. Dr. Hicks believes that even the *Gleocapsa* cells may and do, 'by the condensation or desiccation of the mucous sheath and by the enlargement of the green cell, ultimately revert to the form of the original gonidium from which they arose.' He is disposed to believe that the changes above detailed are by no means all that may occur. Sometimes, indeed, the whole of the gonidial layer of the lichen-thallus becomes converted into a *Palmoglæa*-mass, which, after a time, is indistinguishable from other masses produced from soridia. Observations of a somewhat similar nature, though more limited in extent, had also been previously made by J. Sachs [1]—of which, however, Dr. Hicks was ignorant till at a late stage of his own investigations. These have convinced him that the *gonidium* of a single Lichen may, under the influence of suitable but varying conditions, give rise to products absolutely resembling almost

[1] 'Botan. Zeitung,' Jan. 5, 1855.

every form of *Hæmatococcus* (*Glœocapsa*, Kützing), and also almost every form of *Coccochloris* (*Palmoglæe*, Kützing) as well as that of *Sorospora virescens*—that is to say, from the gonidia of a single Lichen there may be produced, by slight variation of the 'conditions' under which they grow, no less than twenty-three forms, which have been hitherto regarded as distinct species of fresh-water Algæ.

But even this is not all; such algoid forms *may* have other totally different modes of origin, to some of which we shall afterwards allude (see pp. lxiii-lxviii, lxxiii, lxxv, and lxxxviii).

Itzigsohn[1] and J. Sachs[2] had both insisted upon the fact that another group of algoid forms, known as *Nostoc*, originates from a Lichen called *Collema;* and their observations have since been confirmed and extended by Dr. Braxton Hicks. They had observed the development of *Nostoc* from changes taking place in a small ball of jelly-like mucus enclosing two or three beaded cells, which becomes extended from the *Collema*-thallus. Such a jelly-ball may undergo one or other of two developmental phases: first, it may produce continuous colourless threads, and may thus again pass into *Collema;* or, secondly, it may develop no colourless fibres, but the ball may grow in size and transparency, whilst the green-beaded filaments within increase by subdivision, and the heterocysts, common to the *Nostochaceæa*, appear at intervals. This second mode of development, according to Itzigsohn and Sachs, may continue for an indefinite period.

Dr. Hicks describes still another mode by which *Nostoc* may originate from *Collema.* Within the thallus of this Lichen certain gonidia appear, which are larger and of a lighter green than the others. These, when liberated, undergo segmentation for a time, so as to produce a small *Chlorococcus*-mass. Soon, however, the products begin to take

[1] 'Botan. Zeitung,' 1854, p. 521. [2] Loc. cit.

on the *Glœocapsa* mode of growth, and become enclosed in mucous envelopes. In this condition the masses differ somewhat in appearance from the *Gleocapsa* of *Cladonia* origin, and afterwards the differences may become even better marked. They may branch off into different modes of development, and in one of them 'the whole mass, both cell-contents and the mucous layer, are of a dark purple colour; each cell undergoing binary division is surrounded by its own mucous layer, the whole being included in a common one. After a while, the purple coating becomes colourless and fused into one; while the cell-contents become green, and the divisions separate. As segmentation proceeds, the resulting cells assume a linear tendency, till at last a number of moniliferous filaments are formed, having here and there the vesicular cells (*heterocysts*) found in the *Nostochaceæ*.' The passage of *Collema* into *Nostoc* may also, according to Dr. Hicks, take place after different modes; and, moreover, developmental changes of a *precisely* similar nature may be undergone by the gonidia of *Nostoc* itself. He has also described the various steps by which masses of *Nostoc* revert to, or are changed into, *Collema*. Thus the relation between *Nostoc* and *Collema* is shown to be indubitable. But as a matter of direct observation, it is now known that *Nostoc* may also originate even from some of the higher Gymnocarpous Lichens. The gonidia of one of these has been seen by Dr. Hicks developing first of all into Gleocapsoid masses, and then into veritable *Nostoc*-balls, whilst still beneath the apothecia and within the crustaceous thallus. Dr. Hicks is also fully disposed to believe that subsequent researches will tend to support the opinion of Itzigsohn, that *Nostoc* may originate from other Lichens; and that, in fact, all the *Nostochaceæ* may have such a mode of origin.

With regard to the variability of Lichens themselves, the

Rev. M. J. Berkeley says[1] :—'No vegetable productions are more liable to variation than Lichens, a circumstance which makes their study very difficult; and, without a knowledge of the fruit, it is almost impossible to distinguish species accurately. Not only is the crust liable to put on various forms by the over-production of some of its constituent parts, but even where these are in a normal condition, the degree of division of its lobes, the difference of colour, the obliteration of the margin of the apothecia, the exposure of those which have a true excipulum partially covered by the crust, the greater or less crowding of the fruit, the reduction of compound forms to simple, and many other circumstances, induce variations which can only be appreciated by the practised student. The tropical *Verrucariæ*, for instance, assume forms so different, that, without a comparison of the fruit, it is almost impossible to come to any correct judgment, and in these *the Lichenoid character is sometimes completely obliterated* by the non-development or evanescence of the crust.'

Algæ. After speaking of the extreme variability of the simpler unicellular Algæ, Mr. Berkeley also says[2] :—'When we come to the articulated Algæ, amongst which the distinctions of species are often slight, an increased degree of caution is requisite. A very short acquaintance suffices to show the immense difference of diameter which may exist in threads of the same mass, and in the same threads the proportion of length and breadth in the articulations, are quite as variable. Species, therefore, evidently of the most close affinity, cannot be safely separated from mere consideration of relative proportion without any other characters. Even

Loc. cit., p. 166.
Introd. to 'Cryptogam. Bot.,' 1857, p. 418.

the branching of the thread is not sufficient, or the mode of branching. *Cladophora glomerata* assumes a multitude of forms which it would be rash in the extreme to separate, and it may be safely affirmed that of published species of *Cladophora* and *Conferva*, at least one-half will ultimately be reduced. Where *Confervæ* are exposed to drought, they sometimes throw down roots from their joints in search of moisture, a circumstance which must be taken into account in the estimation of species. In *Lyngbya muralis* the threads often anastomose, producing a very curious and puzzling appearance. . . . In *Anabaina* and allied genera, the number and disposition of the fertile cells will not afford safe characters; nor will mere microscopic measurement, which is often deceptive, and should be always taken with considerable latitude amongst *Oscillatoriæ*. The zoospores even of the articulated Algæ are not absolutely constant. Monstrous forms occur in the small zoospores of *Cladophora*, and the large ones of *Ædogonium*. Characters like those in Hassal's Fresh Water Algæ, dependent simply on comparative size, are altogether inadmissible, as specific distinctions.'

From a study of some of the *Oscillatoriæ*, and more especially of *Lyngbya muralis*, Dr. Hicks[1] has not only ascertained that these plants take on many distinctly different modes of growth from time to time, but that they also frequently throw off some of their terminal cells in the form of gonidia, which straightway proceed to undergo a process of segmentation after the fashion of the gonidia of Lichens, so as to produce a palmelloid growth, which frequently assumes the form of *Protococcus viridis*. Some of the cells of these, after a time, may again take on the linear mode of growth, and so revert to the original *Lyngbya* type. At other times, by the occurrence of a lateral mode of segmen-

[1] 'Journ. of Microsc. Science,' 1861, p. 157.

tation, there are produced from the *Lyngbya* thread broad wavy fronds, the cells of which are held together by colourless intercellular substance. Such fronds may also throw off gonidia, and these may go through all the changes above described; or some of its peripheral cells may take on the linear mode of growth. Dr. Hicks says:—'The whole of these changes are so palpable, can be observed so constantly, and are, at the same time, so simple in their relations to one another, that one can scarcely imagine how they can have been separated, not only into distinct species, but into different families of Algæ. Thus the linear stage is called *Lyngbya;* the early stage of collateral segmentation, *Schizogonium;* the adult stage *Prasiola*[1]; while the gonidial growth has been classed under *Palmellaceæ.* And this has been done by most algologists. Meyer, indeed, had pointed out a connection between them; but his opinions were denied by Jesseu, and ignored by most others. It is a striking instance of the insuperable tendency of some to look upon every distinct form as a separate species.' The characters of the cells or segments in *Lyngbya* are subject to changes of all kinds, not only as regards the particular characters of the green colouring matter contained in them, but also as regards the shape and size of the segments. They may be more or less rounded, so as to resemble those of *Nostoc*, and their length not only varies much in adjacent filaments, but even occasionally in different portions of the same filament. This depends altogether 'upon the rapidity of the process of linear subdivision, compared with the rapidity of individual cell-growth. Sometimes the rate of the former is so much in excess, that the cells are no thicker than the septa, the thread appearing to consist of narrow green and

[1] The two latter genera have always been located in the family *Ulvaceæ.*

colourless boards; again, sometimes the cells are three or four times longer than broad.' Much variation occurs also in the width of the whole thread in specimens taken from various localities. Having thoroughly ascertained, therefore, the extent of variation which may undoubtedly occur in *Lyngbya*, Dr. Hicks seems fully justified when he comes to the conclusion that characters of this kind are utterly worthless upon which to found the distinction of species. And yet it is upon such characters only that the so-called species of the genus *Oscillatoria* are separated from one another.

But, it may well be asked, what is the most perfect form of this developmental cycle, in which are included the Algæ known as *Lyngbya*, *Schizogonium*, *Prasiola*, and *Protococcus?* There is no sexual stage of reproduction to be met with in either of them—one can scarcely be said to be higher than another—they are in fact, as it appears, distinct modes of existence, which may be taken on under the influence of suitable condition by one and the same living matter. But can we stop even here? may not these forms themselves be but sub-cycles of a still larger cycle? The gonidia of Lichens, as we have seen, undergo a process of segmentation almost absolutely similar to that undergone by the gonidial cells of *Lyngbya;* we may well ask, therefore, Is it possible that the products of the segmentation of the gonidia of certain Lichens may occasionally assume the *Lyngbya* mode of growth? Each form has of course a tendency to reproduce or continue the mode of growth of the form from which it has been immediately derived; it may therefore continue in almost any one of these stages either for a very long or a very short time, such tendency being extremely weak and not to be compared with that which exists in higher organisms, in which the influence of the 'law of *heredity*' has been gradually built up by the production of similar forms through successive generations. Here the present mode of growth

may be diverted into some other mode under the influence of various conditions at present unknown to us.

But is it possible that just as the algoid cycle of development may merge into the higher Licheno-algoid cycle, so this complex cycle may merge into one still higher, in which the various forms of **Mosses** must be concluded? From what we already know, this is really not so utterly improbable as it might at first appear. Much careful observation would be required to establish such a possibility as a fact, but the observations to which I will now call attention give a certain warrant to the assumption. They afford indications, at all events, which should be sufficient to make future inquirers approach the subject with their eyes fully open to the marvellous possibilities of change presented by the primordial forms of living matter.

Gonidia are produced from almost any part of the thallus of Lichens; but although, in exceptional conditions, they may also be produced from the most different parts of Mosses, they are thrown off principally, and as a rule, from the so-called 'confervoid filaments' or 'confervoid radicles.' The latter are generally to be observed, in more or less abundance, springing more especially from the part where the ascending axis joins the true roots of the moss. These filaments, previous to the investigations of Kützing, were looked upon as true Algæ, and were described under the generic names *Protenema* and *Gongrosira*. Kützing[1], however, showed that they were natural products of Mosses, and this view of their nature was afterwards taken by Schimper[2], who traced more fully the development of the confervoid filaments from the spore, and the origin of the leafy axis of the moss from them. The investigations of Dr. Braxton Hicks have

[1] 'Phycolog. Generalis,' and 'Linnæa,' Bd. viii. 1833.
[2] 'Recherch. sur les Mousses,' 1848.

enabled him to confirm Schimper's observations as far as they went, whilst they also supply us with much new and highly interesting information. These 'confervoid filaments,' Dr. Hicks [1] tells us, 'consist of a single series of cells, of a length varying according to outward conditions [2], each cell possessing the property of producing branches like themselves. They are in the first instance produced by the germination of a spore, as has been pointed out by the observers above quoted; and although, as Schimper has beautifully shown, the ascending axis arises from them, yet this axis and the leaves in their turn give rise to the filaments, as Kützing and Schimper also have pointed out, and which can be readily verified. . . . When the filament springs from either the axis or leaf, or from a single unsegmented gonidium, the first change in the cell (for in either case only one cell is involved) is a bulging-out of a portion of its wall, which, after growing a certain length, is shut off from the original cell by a septum at the point of origin. After this cell has grown a certain length, a binary subdivision of its contents takes place. . . . By the continuation of this process, chiefly in the terminal cell, and by the growth in the already formed cells, and by the formation of branches from branches continuously, the length of the filaments and the area they occupy are extended indefinitely.' The character of the cell-contents varies very much in different cases, and the filaments may take on a well-marked arborescent form, or they may remain, for a period, almost unbranched. Dr. Hicks tells us:—'In these forms the confervoid radicles continue to grow for an indefinite time, external circumstances remaining the same; and in course of time very large surfaces can be

[1] 'Trans. of Linnæan Soc.,' 1862, vol. xxiii. p. 570.
[2] Sometimes their length scarcely equals their breadth; but at other times, 'under much moisture and heat, it is very much increased, so that it may be twenty or thirty times longer than wide.'

covered by them unless usurped by other plants. They are perfectly capable of *independent* existence, whether they have arisen from a spore, leaf, stem, or root, when separated from their source; and hence the erroneous impression of their Algal origin. This is the less to be wondered at, if we notice the growth of one when placed in water. Under these circumstances, the activity of its development and linear growth is wonderfully increased, if not exposed to too much light; so that in a week it can multiply itself 200 or 300 times, while the original type has been nearly preserved, the slight alteration being in the elongations of the cells and a decrease of their breadth.' Sometimes the filaments which have grown in water are branched so as closely to resemble the Alga known as *Draparnaldia tenuis;* and on one occasion Dr. Hicks actually found a very fine growth of *D. tenuis* in a glass of water into which he had previously placed a Moss. Upon this he remarks:—'This is a very unusual place to find this plant; and though I could not absolutely trace it to a Moss, yet coupled with the fact that similar growths can be so originated, and also that the radicles[1] produce elongated cilia-like[2] cells, it seems to be a point worthy of further research, whether or not that genus, or at any rate the above species, may or may not have its origin from Moss in some one of its phases. Nor should this, in our present state of knowledge, be considered a wild speculation, for we know nothing of the agamic growth of *Draparnaldia;* we have nothing to militate against its being one mode of vegetative growth of a form considered altogether distinct; and this is not more extravagant than the known fact that these *confervoid filaments can produce and*

[1] Every transition may be traced between the true radicles of the Moss and the confervoid filaments. Each is capable of giving rise to the other.

[2] Strongly resembling the so-called ciliæ of the *Draparnaldiæ.*

spring from Mosses. I again remark, we know so little of the whole possible life-history of these simpler plants, that our want of knowledge of a precedent cannot be quoted against it.'

Whilst such are the changes that take place in the shade under the influence of warmth and moisture, the growth of the cells is much checked during drought (especially in the summer); their walls become thickened, and the chlorophyll utricles are more closely packed in their interior. Drought also tends to produce a red or reddish-brown colour in the cellulose wall of the cell. Filaments are often (though not necessarily) in this condition when those peculiar buds arise which constitute the first step in the growth of a perfect Moss. This is one of the many modes of reproduction recognizable amongst these filaments.

1. It takes place in the following manner:—From some one cell of the filament a branch is produced, and after one or two dissepiments have been formed in it, the terminal cell increases considerably in size. 'From this,' Dr. Hicks[1] writes, 'many (three or four) branches spring, in the mode of branching before mentioned, in a row or vertical; the cells of these branches are delicate and tapering, and have the property of curving in towards the centre. There is also a similar row of smaller branches springing from the same cell within the former row, surrounding as it were an imaginary axis. From this springs, by gradual increment in the number of the cells, the stem of the Moss. The first attempt at differentiation on the part of the confervoid filament is thus shown to be in the cell producing the rows of curving in branches.'

2. The next mode of reproduction observable amongst these confervoid filaments is also most prone to occur during

[1] Loc. cit, p. 574.

periods of drought. This consists in the formation of the so-called 'peculiar fruit' of the *Protonemeæ*, which may be produced on any part of the plant, under the form of a brownish ovoid mass made up of four or more cells. These are supposed to be 'resting gemmæ,' the production of which is probably 'one of the means by which the life of the plant is preserved during severe trials of drought and cold.' Each one of them seems to be a sort of compound gonidium, and from any one or more of its constituent cells confervoid filaments may ultimately sprout.

3. 'But this is not the only method by which the confervoid filaments are reproduced: any one of the cells detached from the other is capable of continuing the growth of the filament in the same manner as each is capable of doing whilst forming part of the filament, by branching and division, as I have before noticed. And there is a great tendency for these cells to separate from each other, more particularly in the older filaments; but whether old or young they may bulge out on any side, and form a branch which, segmenting, becomes a true filament.' The separated cells may be somewhat elongated, though more frequently they are spherical, having dropped off from the ends of the filaments where three or four others have assumed nearly the same form [1]. The green contents of these separated globular cells, or gonidia, may be granular or homogeneous, and they may or may not contain a central nucleus, as is observed in the gonidia of Lichens. Others become more or less altered in form and appearance, and exactly resemble some of the supposed independent forms of Confervæ described by the older algologists—e. g. *C. multicapsularis* and *C. umbrosa*. Under the influence of warmth and moisture these globular or elongated cells (*a*) give rise to filaments;

[1] These cells also assume a deeper and brighter green colour.

but in other cases (*b*) they become converted into mother-cells by their contents undergoing a process of quaternary subdivision, which repeats itself again and again. This property of repeated and incessant subdivision is thus seen to be common both to the Lichen and to the Moss-gonidium, and, as Dr. Hicks points out, the process is commenced so early in the cell 'that one subdivision is hardly fairly perceptible before the next can be recognized; indeed, in some of the cells three or four generations are included in one parent.' The result of this process in either case may be the production of segmental divisions, 'so small as scarcely to show any distinction between cell-wall and contents;' which, however, 'gradually increase in size, so as at last to equal the original parent cell.' Not only were the products of this subdivision incapable of being discriminated microscopically from those of the Lichen gonidia, but they even resembled them also in their tendency to keep on for an indefinite time in that particular mode of growth and reproduction with which they had commenced. Dr. Hicks says:— 'This can readily be observed by any one who will take the trouble. Large areas may thus be covered by the growth of these cells, which may continue for a long period of time, certainly over a year, and probably, as far as I can make out, for many years. . . . It may always be noticed on the face of any wall where Mosses grow, that underneath each patch a large stream of these Chlorococcus-like bodies may be seen, running downwards.'

4. Sometimes, however, instead of the production of the globular cells from the ends of the filaments, the whole of the cells of a tapering filament may undergo this quaternary form of segmentation *in situ*. The cell-walls become thicker and the filament broader, whilst segmentation goes on. After a time the parent cell-walls dissolve away, and the Chlorococcus-like bodies are liberated. This mode of growth has

been seen more particularly on the bark of trees, and by this means large green patches may be formed.

5. 'But in some filaments there is a still more unsuspected change, namely, in the production of cells of *Gleocapsa*. The segmentation proceeds within the filament, as in the instance just quoted; but the divisions become invested in a gelatinous envelope, while the parent cell-wall breaks up. These Gleocapsa-like bodies then become free, and continue the segmentation process as in *Gleocapsa*. This I have shown in Pl. LVIII. fig. 19 *a*. It is a condition by no means rare in the winter months; considerable masses of these bodies are to be found so produced. . . . I have frequently found *Gleocapsa polydermica* (Kützing) formed, as well as other so-called species. After frequent segmentation, the cells are imbedded in an indefinite mass of gelatinous substance.'

The merely accidental nature, so to speak, of the difference between the *Chlorococcus* product and the *Gleocapsa* product of the confervoid filament of the Moss is well seen by what occurs in certain filaments where there may be a simultaneous production of the two forms side by side. In reference to this, Dr. Hicks writes:—'The cells of a filament in one or in every part at once begin the process of quaternary segmentation, as before noticed, at first regularly, but shortly after irregularly; besides this, a certain amount of free-cell formation goes on within the divisions (mother-cells), so that it is difficult to say which kind of cell formation predominates. In this manner large irregular masses of segmentation cells are produced, like some of those resulting from the segmentation of the so-called Palmellaceæ. The cells set free from them are either Chlorococcus-like cells of variable size, or they are like *Gleocapsa*, undergoing segmentation in their variable manner. . . . These changes can be readily observed in the colder months. They fre-

quently by distortion in all directions, produce a mass whose origin might be very difficult to determine, were it not generally possible to find some small part retaining the original filamentous condition.'

The other reproductive (?) changes described by Dr. Braxton Hicks as occurring in the confervoid filaments of Mosses are of a most remarkable character, and are perhaps more intimately connected with a failing vitality of the mother-cells than those hitherto described. They seem to be instances of true heterogenesis.

6. The whole of the contents of one of the cells may gather together 'into one or more oval masses, which become covered by a cell-wall thrown around each portion.' Then two different kinds of changes may ensue. Either (*a*) they may undergo a process of segmentation whilst still within the parent cell, the products being ultimately liberated by the solution of the cell-walls, containing and contained; or (*b*) the cells may gradually lose their green colour, and assume a colourless appearance save for the presence of a few reddish granules. As these changes advance the units assume the appearance of veritable *Amœbæ*, possessing one or two contractile vesicles, and exhibiting the usual changes of form and movements characteristic of such organisms[1]. After they have moved about for a time as *Amœbæ*, they reassume an ovoid form, and become covered with cilia, which vibrate actively. Further alterations have not been traced.

7. The next change is one commencing in the so-called 'chlorophyll utricles,' which exist not only in the stems and leaves of Mosses, but also in their closely-related radicles and confervoid filaments. They are minute homogeneous granules, or, perhaps more correctly, saccules. Nägeli[2] had pointed out that they were provided with an outer

[1] For details concerning this metamorphosis, see 'Journ. of Microsc. Science,' April 1862.

[2] Ray Soc. 1849, pp. 176–178.

membrane, and that they can multiply by sub-division. The presence of the membrane has been denied by Caspary, Mohl, and Gris, and also by the author of the article on 'Chlorophyll' in the *Micrographic Dictionary*, though the latter admits that they undergo segmentation[1]. With reference to these points, Dr. Hicks tells us that in some moss which he cultivated under glass, 'the various branches threw out numberless confervoid filaments, some of which approached the radicular rather than the confervoid type.' In both these kinds of filaments, however, he observed that their chlorophyll granules possessed the power of enlarging. The granules at first became more consistent on their exterior, at the same time that they became larger. But Dr. Hicks says:—'As they increased they showed a more distinct outline, and it was clear that, whatever doubt might attach itself to the existence of a membrane on the exterior of the chlorophyll utricles of the leaves and ordinary confervoid filaments, *these* contents were enclosed by a delicate envelope; and, as they further enlarged, a nucleus appeared in the centre. After a time, the parent cell broke up, and these once chlorophyll utricles, but now distinct cells, became free. . . . In the undisturbed condition in which they existed, and being held together by the gum-like character of the residue of the parent-cell wall, they, of course, did not spread far; and, as the filaments had attached themselves to the sides of the glass, I had an excellent opportunity of watching their subsequent progress. . . . After increasing gradually in an oval form, they arrived to about the $\frac{1}{1700}''$ in size, when they began to segment into two, or three, or four divisions, or even into more, a nucleus appearing in each division. . . . At this period the cell-wall of the parent-cell (once chlorophyll utricle) was very marked. . . . After

[1] 'Linn. Trans.' vol. xxii. p. 580.

they had remained for some months in a state of complete quiescence, I placed some of these segmenting cells into water on a slide, and, covering them with ordinary thin glass, I put them in the sun for about an hour. To my great surprise, *I found the whole water alive with zoospores.* There were thousands in the square inch, in a most active state. Further examination showed that the segments had been released by the bursting of the parent cell-wall, and had now become these zoospores. After a time they came to rest, and altogether lost their activity. I preserved the slide for some time, but I could not determine anything very definite as to their after-life, beyond that they came to rest, lost their *cilia*, and again subdivided. . . . These zoospores were of a light-green colour, they differed slightly in size, and were principally oval; some, however (and these were the larger), were round. They possessed two cilia, and their contents were granular. The smaller measured $\frac{1}{1800}$ by $\frac{1}{3500}$ of an inch [1].'

So far we have been speaking of the reproductive processes peculiar to, and of the formation of gonidia in, the *confervoid filaments* of Mosses; but the power of producing gonidia is not restricted to such parts. These bodies are occasionally developed from the leaves of Mosses, just as they are capable of being produced from every part of the thallus of a Lichen.

Dr. Hicks says the ascending axis may spring up in the usual manner from a confervoid filament, and the production of leaves may commence; but occasionally the cells, 'which should in the ordinary way unite to form their lamina, in this case do not cohere, but either *run parallel to or branch away* somewhat from each other.' The terminal cell of each of these pseudo-leaves possesses, like the terminal cells of

[1] Reissek of Vienna has also described peculiar changes which are undergone by the chlorophyll of ordinary flowering plants.

the filaments, the power of separating from the others, and frequently it might be noticed that the cells had already begun the process of division before their separation. The production here of gonidia seems to result from a kind of arrest of development.

But Dr. Hicks speaks of still another mode by which free cell elements may originate, when he says:—'I have frequently noticed that Gleocapsa-like cells are produced from the contents of the cells of the older leaves, which, situated at the base of the stem, towards autumn and during winter and spring, have become brown. These leaves are not wholly dead. . . . After a time the old cell-wall dissolves away, and then it becomes evident that the contents have assumed the form of, or rather have become a *Gleocapsa* which certainly undergoes segmentation freely.' He has seen considerable masses of *Gleocapsa* which have had this mode of origin.

Lastly, Dr. Braxton Hicks says[1]:—'It seems to me impossible to discriminate between the cells of the segmenting gonidia of Algæ, of Lichens, and of Mosses; and hence I believe we shall be obliged to conclude that all the cells classed as Palmellaceæ—*Chlorococcus*, *Gleocapsa*, *Sorospora*, and some others, with their so-called species—are but *varieties of one mode of simple vegetative cell-growth, common to most of the Cryptogamia*[2]. What is the value of the differences between each kind it seems difficult to decide, but it may possibly be less than hitherto supposed.'

The transformations of the gonidia of Mosses are, however, far outstripped by the metamorphoses of the antheridia of **Liverworts,** if we are to rely upon the observations

[1] Loc. cit., p. 584.
[2] He has also seen growths of this kind originating by the segmental multiplication of certain spores of *Volvox*. See p. lxxxviii.

of Professor Hartig[1]. The spermatozoids or phytozoa from the antherida of these plants first assume the forms of Ehrenberg's genera, *Spirillum*, and *Vibrio*. They do not last long, since after forty-eight hours all of them are seen to have become disarticulated. The whole drop of water in which they float is then rendered turbid by numberless actively-moving globules, similar to *Monas crepusculum*, which, according to Professor Hartig, are always developed from the broken-up fragments of the *Spirilla* and *Vibriones*. But after forty-eight hours 'groups of several hundred may frequently be seen in which the primary active motion has ceased. Shortly afterwards, a sharply-defined hyaline skin is formed round these groups, and as it would seem, by the amalgamation or conjunction of the exterior molecules. By this means the young *Amœba* (*Proteus*) is formed. This transformation takes place pretty regularly towards the end of the third day. The original size of the *Amœba* is $\frac{1}{500}''$ in diameter.' In the course of three or four days it increases in size, and its movements are very slow. It possesses a vesicle in the posterior part of its body, and resembles the *Amœba princeps* of Ehrenberg. The further changes are thus described:—'After four or five days the *Amœba* assumes a spherical shape, and becomes motionless, the vesicular body expanding and contracting rapidly as before, in a manner similar to what takes place in many *Vorticellæ*. These spherical motionless *Amœbæ* are then, for the most part, united by a mucilage into groups of from ten to twenty. . . . In about a fortnight after the commencement of the experiment a green point appears in the interior of the spherical colourless *Amœba*; this point gradually increases in size till it fills up the entire hollow of the *Amœba*, and, after becoming covered with a cuticle, it

[1] See Translation in 'Journ. of Microsc. Science,' 1855, p. 51.

escapes in the form of an elliptical bright-green cell, $\frac{1}{3000}''$ in diameter, resembling a *Protococcus.* It exhibits a round, transparent cavity, devoid of chlorophyll, corresponding in size and in position to the vesicular body of the *Amœba*, and resembling, at its colourless apex, the motile gonidia of *Cladophora.* A few days later the elliptic or roundish cell lengthens, a formation of transverse septa commences, and the unicellular Alga becomes an articulated one. . . . All these transformations of Phytozoa into *Spirilla*, *Vibriones*, *Monads*, *Amœbæ*, unicellular and articulated *Algæ* may be observed, not only in the detached Phytozoa, but in those which remain in the interior of the sections of the antheridia. In those antheridia, of which the phytozoa are not fully ripe, the *Amœbæ* are seen to originate in the middle of the internal mass of phytozoary cells: some of them make their way out through the softened mass of cellular tissue; but others remain in the interior of the antheridum until their development into an articulated Alga. . . . Contemporaneously with *Amœbæ*, and often earlier, there may be seen, amidst the mass of Monads, bodies very similar in form and motion to the genus *Bodo* (*socialis*), and which increase by transverse division. They have the front end furnished with a long whip-shaped antenna or cilium similar to that of *Euglena.* At their first appearance, their motion, their change of form, and their whole exterior differ so little from the earliest states of *Amœba*, that at this period they cannot be distinguished. In these early stages they both resemble *Chlamydomonas destruens* of Ehrenberg. . . . The above forms uniformly make their appearance, and always in the succession above described. It is true that other forms such as *Uvella*, and even *Leptomitæ* and *Periconiæ*, are sometimes met with, the germs of which may have been imported by the atmosphere during the observation; but these organisms, *which always appear singly*, and after the commencement of

the observation, do not interfere with the above results, when we consider the immense number of the Phytozoa and their uniform and contemporaneous transformations. If about a dozen preparations are made, if they are carefully covered with a bell-glass after each observation, and if care be taken not to extend the observations for too long a time at once, at least half of the preparations will be free from all admixture of foreign organisms.'

Itzigsohn's observations on the developmental changes undergone by certain *Oscillatariæ* have also made known some most startling transformations, concerning which further particulars are given under another section (p. lxxxiii).

Fungi. We have hitherto been calling attention to the variability of Algæ and Lichens, and also to the relations existing between Lichens and Mosses on the one hand, and certain Algæ on the other. Fungi cannot, however, be excluded from this group of related forms. Different opinions have been held by the most eminent naturalists as to whether Lichens are most nearly allied to Algæ or to Fungi—so closely are some of them related to certain forms belonging to both of these families. On the one hand, the mode of fructification of the Lichens, by spore-bearing asci, approximates them to Fungi; whilst on the other, the development of chorophyll—containing gonidia in their interior, and their dependence for nutriment upon the medium in which they exist rather than upon the mere matrix on which they are situated, are thought to affiliate them to the Algæ. The Rev. M. J. Berkeley believes that the relationship is stronger between Lichens and Fungi than between Lichens and Algæ. He says:—'In Lichens, the very simplest display perfect fruit, resembling altogether that of Fungi, insomuch that, of many species belonging to either group, it is almost impossible, in the

absence of crust, to say whether we have a Lichen or a Fungus before us.'

The variability of Fungi under the influence of different external conditions is well known, though the full extent to which this occurs is only gradually being ascertained as a result of special experimental researches on their development.

The Rev. M. J. Berkeley, speaking of the *Hymenomycetes* proper, says [1]:—'The members, however, of which this vast collection of plants is composed, are connected so intimately with one another, and the whole mass is so natural, that it is scarcely possible to say where one genus ends and where one begins. Practice alone, and tact, can do this in the more difficult cases; but it must be confessed that in different states the same species is often positively referable to three or more distinct genera, and it is only the possession of intermediate states which can put one in a condition to say which is the correct nomenclature.' With regard also to the different forms assumed by Fungi, Dr. Lindley says [2]:—'If what is stated above respecting the true nature of ergot be correct, the *Oidium abortifaciens* must be a second form of fruit, in accordance with many facts observed lately in fungi. Fries long ago announced the fact, that several genera supposed to be distinct are in fact merely different modes of fructification, as *Cytispora* of *Sphæria*, *Dacrymyces urticæ* of *Peziza fusarioides*, *&c.* [3] These views were, however, con-

[1] Introduction to 'Crypt. Bot.' p. 357.

[2] 'Vegetable Kingdom,' (3rd edit.) 1853. p. 449.

[3] Speaking on this subject previously, Dr. Lindley said (loc. cit. p. 35):—'That it must be a matter of extreme difficulty to form any precise opinion concerning Fungi, without long experience, will be apparent from the observations of Fries upon the genus Thelephora.' (Elenchus, p. 158.) He asserts that out of mere degenerations or imperfect states of Th. sulphurea, the following genera, all of which he has identified by means of unquestionable evidence, have been constructed, viz., Athelia of Persoon, Ozonium of Persoon, Himantia of Persoon, Sporotrichum of Kunze, Alytosporium of Link, Xylostroma, Racodium of Persoon, Ceratonema of Persoon, and some others. Th. Fr. Nees von

sidered problematical till the matter was taken up by Tulasne, who has collected many facts connected with the subject, while Messrs. Berkeley and Broome have in more than one instance detected two supposed genera growing from a common stroma. *Sphæria inquinans*, for instance, was detected bearing asci internally, and naked spores (Stilbospora) on the outside of the same perithecium. In *Tympanis* also, in the same cup, perfect asci were found by the side of naked didymous spores. The mycelium of *Sphæria Desmazierii* was observed to have distinct spores towards the tips of the filaments, constituting it a true mould. It is very probable that the genera of fungi will be greatly reduced by the continuance of such observations,' and that double fructification will be found as general amongst fungi as Tulasne has found it to be amongst lichens.

Fungi have been classified into genera and families principally on the ground of the fructification which is supposed to be peculiar to them; fungologists are now, however, fast discovering that entirely different modes of fructification may be observed in the same species of fungi at different times. Mr. Berkeley [1] tells us that 'In *Erysiphe* there are no less than five different forms of fruit; the moniliform threads on the mycelium; the asci in the sporangia; the larger stylospores in other sporangia; the smaller stylospores in the pycnidia; and the separate sporules sometimes formed in the joints of

Esenbeck also assures us that the same fungoid matter which produces Sclerotium mycetospora in the winter, develops Agaricus volvaceus in the summer. It would thus seem that the opinions of those who have asserted that the species or genus of a Fungus depends not upon the seed from which it springs, but upon the matrix by which it is nourished, are at least specious; especially if we take the above fact in connection with the experiments of Dutrochet, who obtained different genera of mouldiness at will by employing different infusions. He says that certain acid fluids constantly yield Monilias, and that certain alkaline mixtures equally produce Botrytis.

[1] Introduction to 'Crypt. Bot.,' p. 78.

the necklaces.' This question as to the extreme diversity of the forms of fructification, which may be assumed by one and the same plant under the influence of diverse conditions, has been of late years much studied by the brothers Tulasne, who have embodied their most important observations in a magnificently illustrated quarto work entitled 'Selecta Fungorum Carpologia.'

The lower Fungi belonging to the *Myxogastric* family, which include forms closely allied to the *Saprolegniæ*, are most interesting organisms, more than one half of whose life-history is passed in an amœboid condition. This condition is so obvious, that De Barry has argued in favour of the members of this family being regarded as animals rather than as fungi. The spore cases of *Ethalium septicum* and other Myxogasteres are formed by the union of certain large sarcode or protoplasmic threads. The homogeneous internal substance of this spore case then breaks up into a number of distinct nucleated bodies, each of which, after they have been liberated in water, escapes from a delicate enclosing membrane, 'in the form of a cell, clothed only by a very thin primordial utricle, thus resembling the reproductive cells of many algæ. These escaped cells undergo changes of form, eventually exhibiting one or two cilia, and two or three vacuoles, of which one at least pulsates. They have also a motion of progression and rotation, as in the case of ordinary zoospores[1].' They afterwards appear to develop and to attain a larger size. During this period they lose their cilia and their rotatory movements, and assume the creeping mode of progression and ever-varying form of true *Amœbæ*[2]. De Barry says:—'If, therefore, on the one hand, the development of *Amœbæ* from the products of the germination of the

[1] Quoted from abstract in 'Journ. of Microsc. Science,' 1860, p. 99.

[2] In this condition they are frequently seen enclosing cells of algæ and other articles of food, like ordinary Amœbæ.

spores, and on the other, the production of the sporangia from the sarcode threads, which as regards structure and movements might be described as colossal filamentary *Amœbæ*, be established, it is an obvious conclusion that the latter arise from the farther development of these *Amœbæ*. And this has been confirmed by direct observation in *Æthalium septicum*, *Zycogala*, and *Stemonitis obtusata*. . . . Direct development of the fructifying threads from the Amœbæ produced by the growth of the Zoospores, appears to me beyond a doubt.'

According to De Barry[1] also, one of the white rusts (*Cystopus Candidus*) found on the cabbage, shepherd's-purse, and other plants, develops 'conidia' abundantly, which, when placed in water, yield in from one and a half to three hours a number of actively-moving, biciliated zoospores—produced by a differentiation of the contents of the 'conidia.' After about two hours the cilia disappear, whilst the zoospores come to a state of rest and assume a spherical form. They have never been seen to develop into fungi. Similar active zoospores have also been seen by De Barry to be produced from the 'acrospores' of the potatoe and parsnip-moulds. These also have never been seen to assume the form of fungi. According to De Barry, the production of such zoospores from the acrospores of the potatoe-mould is favoured by the absence of light.

Referring to these and kindred observations, the Rev. M. J. Berkeley speaks in the following terms[2]:—'Few points are of greater significance than those which touch upon the intimate connection of animal and vegetable life. Fresh matter is constantly turning up, most clearly indicating that there are organisms in the vegetable kingdom which cannot be distinguished from animals. The curious observations which

[1] 'Ann. des Sc. Nat.,' 4 Sér. t. xx. (1865).
[2] 'Journ. of Microsc. Science,' Oct. 1868, p. 233.

showed that the protoplasm of the spores of *Botrytis infestans* (the potatoe mould) is at times differentiated, and ultimately resolved into active flagelliferous Zoospores, quite undistinguishable from certain infusoria, have met their parallel in a memoir lately published by MM. Famintzin and Boranetzky[1], respecting a similar differentiation in the gonidia of lichens belonging to the genera *Physcia* and *Cladonia*. It is, however, only certain of the gonidia which are so circumstanced; the contents of others simply divide into motionless globules.'

Phytozoa. Concerning the organisms belonging to this class Dr. Arlidge writes[2]:—'The remarkable phases of existence through which any one species may pass upsets all our notions based on presumed constant characters: for, as we have seen, one and the same being may at one period of its existence exhibit in a preponderating degree the vital phenomena of an animal, at another those of a plant, whence has arisen the hypothesis of the metamorphosis of plants into animals, and *vice-versâ*.' In another place[3] he says:—'In point of fact these organisms stand on the confines between the animal and the vegetable kingdoms,—some genera distinctly belonging to the latter, others doubtfully to the former, whilst many pass through such phases of existence that at one time they assume the characters of animals, at another those of plants.'

Cohn's experiments[4] on *Stephanosphæra* are very valuable, because they show the direct influence of external conditions in modifying the characters of such organisms. We will quote Dr. Arlidge's account of these experiments. He says:—'On placing specimens of this organism, some in

[1] Translation in 'Ann. and Mag. of Nat. Hist.,' Feb., 1869.
[2] Pritchard's 'Infusoria,' p. 128. [3] Loc. cit. p. 111.
[4] 'Nov. Act. Acad. Curios.,' xxvi. 1857.

transparent glass vessels, others in semi-transparent and green ones, others in porcelain, and others again in perfectly opaque cups, the modifications in size and figure according to the intensity of light they received were altogether incredible. In the opaque vessels where they got little light, the green cells remained delicate, small, and widely dispersed, whilst in the transparent glasses, under sunlight, they became many times larger and crowded together, and their figure fusiform, irregular, and produced into numerous protoplasmic processes. Indeed, on placing two portions of the same collection of *Stephanosphæra*-globes, the one in a transparent, the other in an opaque vessel, the swarming individuals in the two will be found so unlike that they might be readily conceived to be different species.'

With regard to the range of variation possible in any one species, the researches of Cohn[1] on the development of *Protococcus pluvialis* have revealed the most startling facts. Summing up the developmental stages, which he had previously given in detail, he says (p. 559):—'Thus we see that a single species, owing to its numerous modes of propagation, can pass through a number of very various forms of development, which have been either erroneously arranged as distinct genera, or at least as remaining stationary in those genera, although, in fact, only transitionary stages. Thus the still Protococcus cell (Fig. 2) corresponds to the common *Protococcus coccoma* (Kütz.). When the border becomes gelatinous it resembles *P. pulcher* (Fig. 70); and the small cells *P. minor*. The encysted motile Zoospore is the genus *Gyges granulum* among the Infusoria, resembling also on the other side *P. turgidus* (Kütz.), and perhaps *P. versatilis* (Braun). The Zoospores divided into two (Figs. 23, 30) must be regarded as a form of *Gyges bipartitus*, or of *P. di-*

[1] See Translation of his Memoir, with plates, in 'Botan. and Physiolog. Memoirs,' Ray Soc., 1853.

midiatus. In the quadripartite Zoospores, with the secondary cells arranged in one plane, we have a *Gonium* (Fig. 37). That with eight segments (Fig. 38) corresponds to *Pandornia Morum*, and that with sixteen to *Botryocystis Volvox* (Fig. 44). When the Zoospore is divided into thirty-two segments, it is a *Uvella* or *Syncrypta* (Fig. 40). When this form enters the "still" stage, it may be regarded as a form analogous to *Microhalola protogenita*; this Algal genus is probably, speaking generally, only the product of the *Uvella* division in the *Euglenæ* or other green forms. The naked Zoospores (Fig. 32), finally, would represent the form of a *Monad*, or of an *Astasia*; the caudate variety approaches that of a *Bodo*. . . . A critical and comparative consideration of the foregoing facts would therefore appear to render untenable almost all the principles which modern systematists have hitherto adopted as the basis for the construction of their Natural Kingdoms, Families, Genera, and Species.'

According to Itzigsohn[1], *Phytozoa* intervene as stages in the developmental history of the *Oscillatoriæ*. The transformations recorded in these observations are of a most remarkable character. The filaments of *Oscillatoria tenuis* are said to break up into distinct fragments which soon assume a spherical shape. These bodies (gonidia) gradually increase in size, become motile, and present in all respects the aspect of individuals belonging to the genus *Chlamydomonas*. After these have attained a certain size, a red eye-point becomes visible in them, and after passing through a great many intermediate forms they develop into perfect *Euglenæ*. These in their turn become encysted, and subsequently by a minute self-division of their contents they are resolved into motile 'microgonidia' which are soon liberated, and then swim about

[1] 'Journ. of Microsc. Science,' 1854, p. 189.

freely. But Itzigsohn says:—'If a number of these remain conjoined, and move about with a rowing kind of movement, their locomotion being governed by a common spontaneity, they represent a *Volvox*-like colony, which, perhaps, may even have been described as *Volvox* by authors. The microgonidia of the *Euglenæ*, like those of all the Algæ hitherto examined by me, are the motile parent cells of extraordinarily minute spiral filaments. They are at first green, gradually becoming pellucid—exactly like the "Spermatospheres" of *Spirogyra*, presenting a monadiform aspect. A peculiar appearance arises when many microgonidia, in such groups, remain green whilst the others have already become clear as water; the mass then presents, in fact, the aspect of being composed of two kinds of animalcules. Such or similar conditions would represent several species of the supposed genus *Uvella* (*atomus*, *glaucoma*, *Bodo*, &c.). Each ultimately colourless microgonidium, then, by the dissolution of its minute gelatinous envelope, discharges a small motile spiral filament. . . . These spiral filaments do not seem to be destined for the purposes of impregnation; for they gradually increase in length and thickness, soon exhibiting numerous spiral turns.' They, in fact, exchange their *Spirilla*-like for a *Spirulina*-form. 'Finally, when their motile faculty has become weakened, they affix themselves by one extremity to any larger object near (e.g. *Conferva* filaments, &c.), whilst the other extremity continues to move about with a creeping motion—the peculiar *Oscillatorian* movement, in performing which a young filament frequently returns to the spiral. The last described condition constitutes the *Leptothrix* of authors. The filaments now gradually become thicker; and though at first of the lightest emerald-green, they gradually assume a deeper and deeper tint. The first indications of articulation are perceptible in them, until at last a young *Oscillatoria* is again perfected.'

According to Dr. G. Gros[1], *Euglenæ* and *Astasiæ* are protean creatures of variable size which undergo the most remarkable metamorphoses. They are capable of giving birth to very varied forms, such as one would scarcely be able to refer to their real origin, if the various transitional stages had escaped observation. These organisms are almost universally distributed, and are capable of giving rise both to animals and plants, under the influence of varying sets of external conditions. Of specimens obtained from the same source, though placed under different conditions, one half may develop into animals, and another half into plants. Dr. Gros says:—'When millions of Euglenæ taken on the same day from the same locality, metamorphose themselves after a certain fashion in one vessel, and after a different fashion in an adjacent vessel, one cannot but place faith in the results, even though one has not actually witnessed [all the steps of] these unaccustomed, though obvious changes—in which there is no room for error, since the transformations take place generally within cysts into the midst of which one could not suspect the introduction of foreign organisms. . . . Why should certain specimens of *Euglenæ* follow one mode of change rather than another? Why, at the first transformation, do they sometimes attain a degree of development to which others arrive only after successive transformations? I know nothing concerning this; but that such phenomena do occur, I am certain. . . . . *Euglenæ* may, on the one hand, chance to produce Confervæ and Mosses; on the other, they may give origin to Rotifers, Nematoids, Tardigrades, &c., according to their size, and to the circumstances in which they are placed—that is to say, according to the quantity and the quality of the substances which they assimilate. . . . In the course of their metamorphoses, they sow, as side

[1] 'Ann. des Sc. Nat.,' 3 Sér. t. xvii. 1852, p. 193.

products, Desmidiæ, Diatomaceæ, Zygnemiæ, and nearly all the vesicular Infusoria, formerly named polygastric. . . . As a general rule, the smaller *Euglenæ* are unable to produce such organisms as are derived from the larger ones; but the larger species, on the contrary, can give birth to almost all the derivatives of the smaller forms. . . . As to the large species, it may be said, with mathematical certainty, that they are the common matrix of almost all the known forms of Infusoria, that they may act as the germs of certain plants, that they produce, as side products, Closteriæ, Diatomaceæ, &c.; that they can succeed in engendering almost all the Rotifers or Systolides, that they can also give birth to Nematoids, Tardigrades, &c. . . . We are not here speaking about theories, more or less logical, but only concerning facts which every skilled observer may ascertain for himself, if he has the good fortune to seize the *Euglenæ* at the time when they begin to encyst themselves, or to undergo transformations at the surface of the water, where their juxtaposed albuminous cysts form a very picturesque alveolar network which may present different stages of metamorphosis, more or less advanced.' Dr. Gros also believes that light exercises a most important influence over the transformations of *Euglenæ*. Where it is absent their developmental modifications rarely go on to the production of plants; the offspring under such conditions are rather animals of one form or another. He has also come to the conclusion, after numerous observations frequently repeated, that different organisms spring from the same matrix according to the mere *quantity* of matter entering into their composition—though of course these variations may be also associated with alterations in quality. He says:—'To give one example of this, out of many others, the substance of certain *Euglenæ* becomes converted into a kind of egg, which has a striking resemblance to the eggs of Nematoids. A certain

number of these eggs, observed for several days in succession (their identity was therefore incontestable), became transformed bodily, some into Rotifers, others into Nematoids. Other eggs presented an individualization of their substance, that is to say, that which, in the preceding case, served to produce a superior organism, became converted into little vesicles which moved about actively within the wall of the ovum, and these little vesicles became as many *Monadinæ*, which escaped from their chamber of incubation.' It is impossible now to follow Dr. Gros into all the details given in his longer memoir[1], which is illustrated by fifteen plates.

In the life-history of *Volvox globator*, according to Dr. Braxton Hicks[2], a portion of its contents, such as would generally be devoted to the production of ciliated gemmules, may occasionally be converted into a large and distinct *Amœba*. Some of the masses, named Zoospores, at an advanced age, instead of undergoing a process of self-division and breaking up into forty or fifty small ciliated bodies, considerably increase in size, and gradually assume an irregular outline. These altered bodies present one or two nuclear-like particles in their interior, and, like *Amœbæ*, they are said to exhibit the most distinct movements, accompanied by changes of form. In a subsequent communication[3] Dr. Hicks writes:—'In the autumn of 1860 I had an excellent opportunity of verifying my notes, and of tracing the change apparently a step further. Out of a large number of Volvox collected in the south of England, I scarcely found one in which the state alluded to did not exist. In many, twenty of these amœboid bodies could be counted moving about immediately beneath the transparent sphere. That they could not

[1] 'Bull. de la Soc. de Natural. de Moscou,' 1847.
[2] 'Journ. of Microsc. Science,' 1860, p. 101.
[3] Ibid. 1862, p. 96.

have derived their origin from any similar or dissimilar existences outside the Volvox, nor could have themselves entered within the sphere by solution, became very evident from *observing the different stages by which the ordinary zoospore gradually arrived to the condition now under discussion.*' After the altered amœboid zoospore had begun to travel, 'it was always noticed that for every one such moving body in the Volvox there was the empty space of a missing zoospore.' A similar amœboid change may also overtake such zoospores as have gone on to the first stages of gemmule growth.

In other cases a very interesting deviation from the ordinary gemmule growth occurs, which has also been described by Dr. Braxton Hicks. As a rule the zoospore divides so as to lead to the formation of about thirty-six cells within the common cell-wall. 'These,' Dr. Hicks says, 'in the ordinary way would pass on to further sub-division, producing almost from this point ciliated cells, which, again re-dividing, would produce the ultimate zoospores held together by the hollow, spherical membrane—or, in other terms, the ordinary Volvox. Instead, then, of the sub-division forming the ciliated cells, which tend towards the exterior of the mass, motionless spores or gonidia are produced, which do not tend outwardly, but which retain their position, except that they become more separated from each other by the increase of the intervening mucus. Watching these throughout the period above mentioned, I found that the segmentation continued in various modes till the masses became one-eighth of an inch in diameter, preserving more or less of a globular form, but indefinite so far as any investing membrane was concerned. . . . At first the division went upon the binary plan[1]; after which some of them divided into

[1] Dr. Hicks has represented these various changes on Pl. IX. of 'Journ. of Microsc. Science,' 1861.

three or four segments—the division being cruciate—while others extended themselves in a linear series, with their short diameters in a line. . . . Some of the divisions, instead of sub-dividing, increased in size, producing a green cell much larger than the rest. . . . . The mucus which formed around these cells was at first more or less definite in boundary, but after segmentation had advanced to some degree its outline was irregular, and at last quite indefinite. The outer edge never possessed more solidity than the mucous envelope of *Cladonia gleocapsa.*' Dr. Hicks has seen this condition commence *within* the parent *Volvox*. He points out its resemblance to that which occurs occasionally during the growth of *Pandorina* and *Stephanosphæra*, and he adds:—'There is also a striking analogy between these and the segmenting gonidia of lichens, especially of *Cladonia*.'

Other additional modes of reproduction in *Volvox* are described in Pritchard's 'Infusoria.'

Mr. H. J. Carter believes that *Euglenæ* and *Astasiæ* are closely related to the members of the genus **Amœba**. He has seen the actual transition of *Astasiæ* into *Amœbæ*, and says[1]:—'Young *Astasiæ* are developed within the cells of *Spirogyra* to a great extent; and although they at first have almost as much polymorphism as an *Amœba*, still they retain their cilium, and after a while assume the form and movements peculiar to *Astasia*. I might here mention that on one occasion I saw a *large Amœba* with a long cilium, at one time assuming the form of *Astasia*, and at another that of *Amœba*, which thus gives us the link between these two Infusoria[2]. The

[1] 'Ann. of Nat. Hist.,' 1856, vol. xvii. p. 115.

[2] I have observed this modification very frequently. Whenever the *Astasia* is hemmed in by opposing particles or fragments, it insinuates itself between them and moves generally after the fashion of an *Amœba*; but, as soon as it has freed itself from the obstacles, it resumes its more active mode of locomotion.

cilium, however, had not the power of the filament of *Astasia*, though it occasionally became terminal.' But Mr. Carter has also seen ordinary *Amœbæ* actually developed from *Euglenæ*. He says:—'I was led to notice this development by an apparent metamorphosis of the cell contents of some fixed and capsuled *Euglenæ* into granuliferous *Amœbæ* of a pinkish colour, within the old cell of *Euglena* itself; and the presence of several such *Amœbæ* creeping about the watch-glass, while many of the cells of the *Euglenæ* (*viridis?*) were empty, or only contained a little effete matter, left no doubt in my mind as to the origin of both colour and infusorium. It was also observed in some instances, where the contents of the *Euglenæ* had passed into an amœbous mass, that the latter underwent a kind of segmentation, so that several (perhaps eight) small *Amœbæ* were developed instead of one large one.'

Speaking of *Amœbæ*, Nicolet says[1]:—'The substance which forms the body of the Amœbæ may be regarded as the fundamental matter, the constituent substance, of that of all the Infusoria. In short, the greater number of these animalcules reduce themselves into Amœbæ, and live under this form when the conditions of their medium, and especially the temperature, are not favourable to another mode of development; and if, as a result of a constant uniformity in the most essential conditions of their existence, an infusorial animalcule happens to reproduce itself by germs, it is always in the state of a Monad (primitive form of the Amœba) that it recommences its life circle.' Then, again, with reference to the establishment of distinct species of *Amœbæ*, such as naturalists have hitherto described, he says:—'The different forms which the Amœba assumes in order to effect its movements have been used as specific

[1] 'Arcana Naturæ,' p. 24.

characters by the major part of the microscopists who have observed them. Nevertheless, these forms, as well as the particular size, which is often dependent upon accidental circumstances, have no specific value. We shall see farther on, that if a division into species can be established, it could only have for its basis the mode of development, though even this is found to be subjected to so many variations, attributable to the modifications of the medium in which the animals are found, that it would be more natural to admit only a single species, and to consider as simple varieties the slight differences which result from their mode of development.'

According to Lieberkühn[1], a relationship exists between *Amœbæ* and *Gregarinæ* which was previously unsuspected, though similar in kind to the relationship described by Carter between such organisms and *Astasiæ*. He says[2] he has seen the conversion of *pseudo-Navicellæ*, that have been undoubtedly derived from *Gregarinæ*, into *Amœbæ*, and he states that he has also met with every transition between such *Amœbæ* and perfect *Gregarinæ*. The transition commences by the case or cyst-like wall of each *pseudo-Navicella* rupturing and giving exit to the soft contained matter, which at first much resembles a minute *Amœba*, but gradually assumes, by progressive growth and the formation of an investing membrane around it, the characters of a *Gregarina*.

There has been much dispute as to whether the *Gregarinida* are to be considered as independent organisms, or merely as embryonic phases of other beings. Kölliker and Leydig[2] have advocated the notion that they are metamorphic stages of certain nematoid worms. But even then there is a further dispute as to whether the *Gregarina* becomes

[1] 'Mém. de l'Acad. Roy. de Belgique,' 1854, t. xxvi.
[2] In 'Journ. of Microsc. Science,' vol. i. p. 208, some of Leydig's arguments are given.

changed into a *Filaria*, or whether *Filaria*-like worms are converted into *Gregarinæ*. Leydig at first held the former view, but now, in common with Henle and Bruch, he inclines to the latter, since otherwise it would seem impossible to account for the undoubted production of *pseudo-Navicellæ* and *Psorospermiæ* within *Gregarinæ*. The development of the Nematoid from the *Gregarina* would seem to be much more possible than the reverse process. And if it chanced to be a kind of metamorphosis which only occasionally happened (Dr. Gros), then its occurrence would be quite compatible with the production of *pseudo-Navicellæ* in other cases.

But whether the developmental cycle of the *Gregarinæ* is, in all cases, such as has been indicated by Lieberkühn, or whether they are occasionally related to the Nematoids, it seems almost certain that the peculiar bodies known as *Psorospermiæ* are more or less analogous to the *pseudo-Navicella* progeny of the *Gregarinidæ*. Professor Huxley says[1]:—'The *Psorospermia* are pyriform sacs, frequently provided with an elongated, filiform, motionless appendage, and containing two or four clear rounded bodies, attached side by side, within their smaller ends, and besides these, as Lieberkühn has lately pointed out, a rounded mass of plasma. Under fitting conditions the *Psorospermia* bursts, and the plasmatic mass emerges as an amœbiform creature. The sacs in which the *Psorospermiæ* are developed, on the other hand, can be traced back to amœbiform masses full of granules; and it seems a legitimate conclusion that the *Psorospermia* are the *pseudo-Navicellæ* of an Amœbiform *Gregarina*, or Gregarinoid *Amœba*.'

The naked *Amœbæ* are, however, closely related to **Arcellinæ**, which are simply *Amœbæ* inhabiting a single-chambered shell, and these again are connected by almost

[1] 'Med. Times,' 1856, xxxii. p. 508.

insensible gradations with the **Foraminifera**, in which the shells have many chambers. Speaking of these creatures, Dr. Carpenter says[1]:—'The range of variation is so great among *Foraminifera*, as to include not merely the differential characters which systematists, proceeding upon the ordinary methods, have accounted *specific*, but also those upon which the greater part of the *genera* of this group have been founded, and even in some instances those of its *orders*. . . . The ordinary notion of *species*, as assemblages of individuals marked out from each other by definite characters that have been genetically transmitted from original prototypes similarly distinguished, is quite inapplicable to this group; since even if the limits of such assemblages were extended so as to include what would elsewhere be accounted genera, they would still be found so intimately connected by gradational links, that definite lines of demarcation could not be drawn between them. . . . The only natural classification of the vast aggregate of diversified forms which this group contains, will be one which ranges them according to their direction and degree of divergence from a small number of principal family types.'

Between *Amœbæ* and the **Actinophryna**, again, there are the closest affinities. On this subject Nicolet says:—'In its different modes of development the *Amœba* often assumes the radiated form of the *Actinophrys*, and this so exactly, that the greatest attention is needed to distinguish the one from the other; also all the authors who have concerned themselves with the classification of these Microzoa, and with the determination of their generic forms, have always confounded the radiated *Amœba* with the *Actinophrys* properly so called. It will suffice to mention *Actinophrys*

[1] Introduction to 'Study of Foraminifera' (Ray Society), 1862, p. x.

discus, *digitata*, *viridis*, and even the *Actinophrys sol* of some authors, all of which are veritable Amœbæ, to make it evident that the rayed aspect does not suffice to characterize this genus, and that we must seek for its determination characters which are more precise.'

In addition to this resemblance existing between *Amœbæ* in their encysted condition, and *Actinophrys*, it seems to be quite certain that many *Acinetæ* so closely resemble members of the family *Actinophryna* that they are often mistaken for one another. All the contradictory statements made by some of the best observers as to the presence or absence of an investing membrane in *Actinophrys* seem only explicable on some such supposition [1]; though it seems extremely probable that these several forms are separated by no distinct line, and that transitions may take place from one to the other type. According to Stein, indeed, there is an actual and close relationship between *Acinetæ* and *Actinophrynæ*, since both are looked upon as occasional developmental phases of *Vorticellinæ*. Cienkowski, moreover, says [2] that the organism described as *Actinophrys* by Ehrenberg is really a non-pedunculate *Acineta*, and he also remarks that, although numerous points of relation exist between certain *Acineta*-forms and *Podophrya fixa*, he is unable to determine whether they should be regarded as identical, or as extreme links in the morphological cycle of one and the same species [3].

[1] See Pritchard's 'Infusoria,' 4th ed. p. 244.

[2] 'Journ. of Microsc. Science,' 1857, p. 101.

[3] Cienkowski witnessed the process of fission taking place in *Podophrya*. It was completed in about half an hour, and during this time he states that he distinctly saw a temporary formation of cilia—these making their appearance and then disappearing after an existence of twenty minutes. He says:—'During the process of division both segments were furnished with tentacles; but when the oscillations of the cylindrical portion commenced, very fine and short cilia might be seen, though with difficulty, vibrating on the free end,—the tentacles at the same time being retracted and remaining visible only on the posterior segment. I now followed uninterruptedly the movements of

Ciliated Infusoria.—Stein declares[1] that within the body of *Actinophrys sol*, and also within that of *Podophrya fixa*, a ciliated germ is often produced, which subsequently develops into a *Vorticella*. Cienkowski believes that the *Actinophryna* of Stein were really *Acinetina*, though he also declares he has seen the development of ciliated embryos within the latter, which however give rise, not to *Vorticellæ*, but to organisms similar to those from which they have arisen. Stein would make *Acinetina* a mere developmental phase in the life-history of *Vorticellina*, whilst the observations of Cienkowski would tend to establish them as a distinct and separate family. Mr. Carter's[2] observations, again, are partly in accord with those of Stein, since he says he has frequently seen the passage of *Vorticella* into *Acineta;* though they are otherwise in agreement with those of Cienkowski, since he says he has never seen the young of *Acineta* assume any other form than that of *Acineta*.

Stein believes that encysted *Vorticellæ*, after undergoing certain metamorphoses within their cyst-wall, may be converted, according to the influence of external conditions, either into an *Actinophrys* or into a *Podophrya*. The creatures so produced are then thought to give birth to a ciliated embryo, which is at first lodged within a definite cavity in the substance of their body. The embryo is pear-shaped, with a central constriction, and it is provided with one or

the liberated segments. . . . Cilia could not be perceived over the whole surface. . . . After waiting patiently for twenty minutes, I saw the motion cease; and, at the same time, short tentacles made their appearance, which were protruded more and more; and in a few minutes afterwards the segment regained the spherical form: thus, after moving about freely for a time, it was again transformed into a *Podophrya*.' This was witnessed, Cienkowski tells us, by other observers as well as himself, and the phenomena are in some respects comparable with what takes place in the formation of the ciliated spore of *Vaucheria*. (See vol. i. p. 175.)

[1] 'Die Infusionsthiere auf ihre Entwickelungsgeschichte,' Leipzig, 1854.

[2] 'Ann. of Nat. Hist.,' 1857, vol. xix. p. 260.

more rows of cilia, by means of which, before effecting its exit, it moves about in the cavity in which it is contained. Its body is made up of a finely-granulated sarcode, containing a band-like nucleus in its posterior portion, and closely adjacent to this there is a distinct contracting vesicle. No mouth is visible, but on the whole it presents a form which would seem easily convertible into a *Vorticella;* and this Stein believes to be its next developmental stage.

A more or less similar life-history is, he believes, the rule with very many of the Ciliated Infusoria. Cienkowski, Kölliker and others, however, believe that Stein has confounded certain *Acinetæ* with members of the family *Actinophryna*, and although they fully admit that *Acinetæ* do develop ciliated embryos in their interior similar to those described by Stein, Cienkowski and some others believe that such ciliated embryos are reconverted into *Acineta*-like organisms, and that they do not give rise to *Vorticellæ* as alleged by Stein. Cienkowski's observations[1] on one occasion were undoubtedly sufficiently positive to entitle him to come to the conclusion that 'from the *Acineta*-embryo, after a prolonged motile stage, another *Acineta* is formed,' but, as he himself admits, his 'observations do not, of course, show that it is impossible that the motile *Acineta*-embryo should be transformed into a *Vorticella*, and a *Vorticella*-cyst into an *Acineta*.'

Cienkowski's observations would only really militate against those of Stein, if we were to assume, what is highly improbable, viz., that the developmental metamorphoses amongst this group of animals are in all cases the same. Much evidence exists, pointing to the conclusion that there is the greatest variability on different occasions, and, consequently under the influence of different sets of con-

[1] 'Journ. of Microsc. Science,' 1857, p. 96.

ditions, in the precise nature of the developmental changes or transformations observable amongst these organisms. In fact, Stein himself has described a second mode of origin of embryos within *Acinetæ* of *Vorticella* origin. Such *Acinetæ* did not possess the ordinary tentacles, though they presented one or two short, closed, tubular processes projecting anteriorly. Within, there was no longer the usual granular contents with a nucleus and contractile vesicle: there were instead, six oval, cell-like bodies, about $\frac{1}{50}''$ in length, which seemed to have been developed out of the original contents of the *Acineta*. These bodies were sharply defined, and contained a coarse granular substance with a contractile vesicle. In one of them a ciliated furrow was observed, owing to the presence of which it more closely resembled the usual solitary embryo of the *Acineta*. Such a multiple development seems to be only an occasional modification of the reproductive process.

A glance at the other modes of reproduction which have been described amongst the Ciliated Infusoria will astonish the reader by their diversity, and will almost force him to come to the conclusion that there is no definite method of reproduction for any one species. There seem to be, rather, different developmental possibilities by any one of which the reproduction of individuals may be brought about, though the particular mode which is likely to occur on any given occasion appears at present to be wholly uncertain. But the actual process gone through—although oftentimes apparently the work of chance—would doubtless be capable of predication, if we knew what were the molecular forces at work in organisms of different kinds, of different sizes, and of different ages, and as well the degree and nature of the modifying influence exercised by the ever-varying sets of conditions to which these different organisms are subjected, under all their widely differing states of molecular activity.

I will, therefore, classify, as well as possible, the various methods of reproduction that have been recorded as occurring amongst one or other of the *Ciliated Infusoria.* But it should be understood that although particular modes of reproduction may be very frequently met with in individuals having a given form, on the other hand the greatest latitude is possible. Different representatives of any supposed species may, occasionally, under the influence of different external or unusual internal conditions, produce organisms either similar or dissimilar to themselves by five or six distinct methods.

Modes of Reproduction amongst Ciliated Infusoria.

I. Reproduction by means of **Fission.**

A. Animal in the *free* state.

Fission may be either *longitudinal*, *transverse*, or *oblique* in its direction; though individuals of the same species may occasionally divide in different directions.

a. Animals in their adult or mature condition—this being most frequently the case.

b. It may occur even in young embryos produced from fission of pre-existing individuals in an encysted state; e.g., in young thus produced, of *Stylonichia pustulata*, which closely resemble *Trichoda lynceus* (Cienkowski). The creatures so produced occasionally encyst themselves before undergoing any further development.

B. Animal in the *encysted* state.

a. Production of embryos which are more or less similar to the parent.

One of the best examples of this mode of reproduction is met with in *Colpoda cucullus*, which seems to divide only when in the encysted state. As a general rule, the process of fission only occurs once, but it may be repeated, so that four, eight, and even sixteen segments are produced. There is also the further peculiarity, that such segments generally encyst themselves while still within the parent cyst.

Stein also speaks of the development of embryos in certain *Acinetæ*, after an exceptional method: the substance of the encysted animal appeared to give rise, by a process of division, to six or more embryos[1].

b. Resolution of the *fission*-segments into 'brood cells,' containing a multitude of monadiform ciliated germs[2].

This process has been observed by Stein in *Vorticella microstoma* and *V. nebulifera*, and by Cienkowski in *Nassula viridis*.

The differences between the cases ranged under A and B respectively are rendered much less absolute by the fact that *Vorticellina* undergo the process of fission when they are only one step advanced towards encystment—that is to say, after they have withdrawn their ciliary apparatus and contracted their body into a more or less rounded or oval state, though previously to the production of a cyst.

[1] Pritchard's 'Infusoria,' 4th ed, p. 364. This is the process referred to at p. xcvii.

[2] See Pritchard's 'Infusoria,' pp. 357, 358. There are good reasons for believing that some of these tubulating 'brood-cells,' so produced, develop into fungi belonging to the genus *Phytbium*.

II. Reproduction by means of **Gemmation**, is a process which also slides by the most insensible gradations into the process of fission. All intermediate grades may be traced between the production of the most obvious bud-like projection, and the process by which a single individual is divided into two equal segments. In ordinary fission the 'bud' is equal in size to the parent organism (which is diminished to half its bulk), and each segment obtains half of the original 'nucleus.' In ordinary gemmation, however, the new individual is produced rather by a growth from, than by a division of, the parent organism, and an entirely new 'nucleus' is evolved within the bud. The process of external gemmation, amongst the Ciliated Infusoria, is much rarer than that of fission.

The buds or gemmæ behave very differently according to the species under observation, and also, to a minor extent, even among individuals belonging to the same so-called species. Some develop a posterior circlet of cilia and swim about for a time; they may then (*a*) either develop a stalk or sheath and resume all the characteristics of the parent-stock, or (*b*), becoming quiescent, they may *encyst* themselves preparatory to further development. The young gemmæ of *Spirochona* differ from those of the *Vorticellina* generally, by the non-development of the posterior ciliary wreath; whilst a development of cilia (lasting only for a time) takes place anteriorly, before the gemma assumes the parent form. In other buds of this species, however, according to Stein, a process of encystment at first takes place, during which the gemmæ are converted into very peculiar Acinetiform beings, usually known as *Dendrocometes paradoxus*.

The process of budding observed by Stein in *Lagenophrys* presents many peculiarities, and, after alluding to his

description, Dr. Arlidge says[1]:—'If this account be correct, the gemmation of *Lagenophrys* is actually a compound process of budding and fission, whilst the resultant beings differ widely from those of other *Vorticellina* in all details, and are so very aberrant in form from the parent, that they require to undergo a metamorphosis before they gain it.'

III. Reproduction by **formation of Embryos out of the substance of the 'Nucleus.'**

A. Such development occurring during and after *conjugation* of two individuals only (Balbiani).

a. Embryos may be developed, not in the whole nucleus, but in *portions* of this body which have been separated by previous fission.

These generally occur in small numbers; and in *Paramecium bursaria*, only one of them undergoes development at the same time[2]. Such an embryo makes its way out of the parent as a tentaculated *Acineta*, devoid of cilia.

b. Or the *whole* of the nucleus may break up, preparatory to the development of embryos throughout its substance.

In this case, the embryos may be either few or many. According to Balbiani, in the genera *Stylonichia* and *Urostyla* the embryos are always four in number, the nucleus being double in each genus. Each nucleus divides, and each half of it is metamorphosed into a single embryo. In the elongated nucleus of *Spirostomum ambiguum*, however, about forty or fifty

[1] Pritchard's 'Infusoria,' 4th ed., p. 353.
[2] 'Ann. of Nat. Hist.,' 1858, vol. i. p. 435.

embryos become developed during the period of conjugation [1].

B. Such development occurring in single individuals, independently of conjugation.

a. Animals *free* and active.

1. Development of embryos out of portions of nucleus, separated by fission or gemmation, has been seen in *Nassula* and in *Paramecium*. Four or five embryos are often produced at the same time by this method, though they are discharged singly. Cohn says that, instead of the ordinary Acinetiform embryos, he has frequently witnessed the escape of others, having a globular figure, which, in addition to the tentacles, are provided with cilia over their whole surface. He has, on one or two occasions, seen a fission of the parent animal take place during the very time that embryos of this kind were escaping.

In *Nassula elegans* Cohn has also seen embryos developed in a cavity within the body, which was adjacent to the nucleus, and communicated by a narrow canal with the exterior [2]. After their escape the embryos were motionless and without cilia, though they presented a few knobbed tentacular processes. A similar mode of development of embryos has been observed in the products of a recent fission

[1] 'Ann. of Nat. Hist.,' 1858, vol. ii. p. 439.
[2] 'Zeitschrift,' 1857, p. 143, and Pritchard's 'Infusoria,' p. 356.

—when the parents were still only half their natural size.

2. The whole substance of the nucleus may undergo successive segmentations, so as to be entirely converted into embryos. The number of embryos produced is, however, very variable, not only in different species, but, according to Lachmann[1], even in the same species. The size of the embryos is always inversely proportionate to their number.

b. Animals *encysted* and stationary.

Single, or several successive embryos, may be produced within a cavity occupying the situation of the nucleus.

This mode of origin of embryos has been described by Stein as occurring in *Chilodon cucullus.* The embryo differs much in appearance from its parent, and is precisely similar to *Cyclidium glaucoma.* Individuals of all sizes undergo the process of encystment, and embryos of proportionate size may be produced in any of them.

After having given birth to one or more embryos, some *Chilodons,* according to Stein, pass from their quiescent into an active state.

The production of *Acinetæ,* as one of the stages in the developmental history of the *Vorticellina,* also takes place by the process of which we are now speaking. This

[1] 'Ann. of Nat. Hist.,' 1857, vol. xix. p. 332.

evolution, however, occurs in these creatures, as previously explained, at a more advanced stage of their developmental cycle.

IV. Reproduction by the **formation of Germs,** in the substance of the organism, which have no genetic relationship to the 'Nucleus.'

a. According to Perty, ovules are produced in large numbers in certain Infusoria. He says he has seen a specimen of *Amphileptus moniliger* distended with from 100 to 150 minute germs. *Paramecium versutum* has also been seen by the same observer to contain a number of greenish ovules. (Mr. Carter, and a few others, adopt these observations of Perty, though the larger number of naturalists seem inclined to throw doubt upon them, or to deny that such bodies are ovules.)

b. Eckhard recorded[1], with great precision, the mode of evolution and discharge of three ovules, in *Stentor cæruleus,* which made their appearance as minute granular globules. He described the mode of formation of the mouth, the evolution of cilia, and the appearance of contractile vesicles. These observations have been confirmed by Oscar Schmidt. In *Stentor polymorphus,* Eckhard has also seen similar globules within the body, though he has not watched their development and exit.

[1] 'Ann. of Nat. Hist.,' 1846, vol. xviii.

c. One or more germs appear to form occasionally in animals which are about to conclude their existence. Their formation seems to be the last vital act on the part of the parent organisms, which gradually perish as the development of their offspring progresses.

1. According to Nicolet[1], an ovum forms, under these circumstances, by the aggregation of previously scattered granules, and the subsequent development of a membrane around such a granule heap.
2. Pouchet also speaks[2] of the presence of ova, under such circumstances, in specimens of the genera *Kolpoda* and *Keronea*, though he does not seem to have traced their mode of formation.
3. Lastly, J. Haime states that he has seen a body of this kind appear shortly before the death of *Paramecium aurelia*, which very soon broke up into about sixty minute ovules or zoospores. These increased in size, burst the sac in which they were contained, and afterwards escaped from the disintegrating body of the parent through a rupture in its integument.

Thus almost every conceivable mode of reproduction seems to take place by turn in these creatures, and the developmental modifications which they have been seen to undergo are so many and so various as to make it almost

[1] 'Arcana Naturæ,' p. 30. [2] 'Hétérogénie,' p. 400.

impossible to say what amount of metamorphosis may take place. Analogical evidence appears to be of no avail when applied to such utterly variable creatures as these. We are almost at the mercy of individual observers, and it seems almost impossible for any of their statements to be checked by saying that such or such a developmental cycle is an impossible one. From what we already know on the best evidence, we obtain a basis of so shifting a nature, that he would be a bold man who would dare to say, on *à priori* grounds, that any particular, alleged, developmental metamorphosis could not take place. Almost everything must turn upon our faith in the accuracy of the observer.

Many other very startling modifications, to which I have not alluded, have been described amongst the Infusoria, in which, more especially after a process of encystment, one form has given place to another, belonging even to what has been reputed a totally different genus [1].

Such statements are, moreover, to a great extent, countenanced by the fact that in organic solutions of a given kind there is almost always a definite order of succession observable amongst the Ciliated Infusoria which it contains. This is a matter of notoriety, and most of the other explanations which have been offered are certainly by no means convincing. To give one example of such succession, we may quote Dr. Arlidge's [2] account of Cohn's observations. He says:—'In a vessel containing decomposing *Spirogyra*, at first appeared countless specimens of *Paramecium aurelia;* these were replaced by the *Proteus* of Baker, either the *Lachrymaria proteus* or the *Trachelocera olor* (Ehr.); these in their turn were followed by *Chilodon cucullus*, and after

[1] See the observations of Pineau in 'Ann. des Sc. Nat.,' 1848, 3 Sér. p. 99; of J. Haime, in 3 Sér. vol. xix.; and of Gros in 'Bullet. de la Soc. de Natural. de Moscou.'

[2] Pritchard's 'Infusoria,' 4th ed. p. 372; or Cohn in Kölliker und Siebold's 'Zeitschrift,' 1851, p. 258.

a few days by a *Kolpoda;* afterwards large *Euplotes* with prominent green globules, probably a new species; and lastly, colourless specimens of *Euplotes charon*, exhibited themselves,—all the species following each other in succession in the course of three weeks, a new form appearing on the decline of a preceding, attaining its maximum in number, and then decreasing in its turn to make room for the next in the series.'

But can we even say that the developmental cycle is confined to Amœboid and Infusorial forms? Very much uncertainty undoubtedly exists on this subject, and the statements of Agassiz, Gros, and others, however incredible they may appear, should, we think, be kept in view as possibilities, rather than summarily rejected on account of any *à priori* considerations. Agassiz maintains that the Ciliated Infusoria have no title to exist as a distinct class. 'Most of them,' he says [1], 'far from being perfect animals, are only germs in an early stage of development. The family of the *Vorticellæ* exhibits so close a relation with Bryozoa, and especially with the genus *Pedicellina*, that I have no doubt, that wherever Bryozoa should be placed, *Vorticella* should follow and be ranked in the same division with them. *The last group of Infusoria*—Bursaria, Paramecium, *and the like—are, as I have satisfied myself by direct investigation, germs of fresh-water worms*, some of which I have seen hatched from eggs of *Planaria* laid under my eyes [2].'

But even if such observations are perfectly correct and true, the evolution of a Ciliated Infusorium, out of the egg of a *Planaria*, would by no means necessarily carry with it the counter proposition, that such Ciliated Infusoria were them-

[1] 'Ann. of Nat. Hist.,' 1850, vol. vi. p. 156.

[2] Mr. Girard confirms these statements, and maintains that *Kolpoda cucullus* is one of the embryonic stages of fresh-water *Planariæ.*—'Proceed. of American Associat.,' 1848, p. 402.

selves capable, after one or several developmental stages, of being converted into *Planariæ*. There might possibly be a reversion of the comparatively undifferentiated matter of the ovum, after it had undergone certain unusual changes, into an organism of a lower type; but though such an outcome from the egg of a *Planaria* might be, as it were, an occasional and accidental occurrence, it could not on this account alone be regarded as one of the regular stages in the life-history of *Planariæ*.

APPENDIX E.

On the 'Germ-theory' in relation to Epidemic and 'specific' Contagious Diseases[1].

In medicine, even more than in other less complex sciences, it is well that imperfectly established general doctrines should be, from time to time, tested by the light of more recently acquired facts. Practice necessarily follows along the paths indicated by theory, and therefore it is in many cases all-important, even from a practical point of view, that true theories should be arrived at. The wider the applications of the theory, the greater is the necessity that it should be sound, and based upon the best knowledge of the time.

I have determined to lay before you some considerations touching the nature and origin of epidemic and so-called 'specific' contagious diseases; and you will be impressed with the vast importance of the subject when you learn that nearly one-fourth of the total number of deaths occurring in Great Britain are due to these affections. As the Registrar-General has aptly pointed out: 'Diseases of this class distinguish one country from another—one year from another; they have formed epochs in chronology; and, as Niebuhr has shown, have influenced not only the fall of cities, such as Athens

[1] Being the Inaugural Address in the Faculty of Medicine of University College, which was delivered on October 2nd, 1871. The first and last paragraphs have been omitted, whilst explanatory notes have been added.

and Florence, but of empires; they decimate armies, disable fleets; they take the lives of criminals that justice has not condemned; they redouble the dangers of crowded hospitals; they infest the habitations of the poor, and strike the artizan in his strength down from comfort into helpless poverty; they carry away the infant from the mother's breast, and the old man at the end of life; but their direst eruptions are excessively fatal to men in their prime and vigour of age. They are emphatically the *morbi populares*.'

No labour is too great, then, no pains should be spared, in order to arrive at just conceptions concerning the origin, nature, and mode of distribution of these scourges of humanity. Deeply impressed with the difficulties surrounding these great problems, and with the enormous importance of strengthening the foundations of our knowledge in respect to them, I was induced rather more than two years ago to take up the investigation of some questions which lay at the root of the whole subject. It seemed to me that no real advance could take place in our power of controlling these diseases until certain other great problems had been settled. What is the real cause of fermentation and putrefaction? Can the organisms which are associated with many of these processes arise *de novo?* These were questions the solution of which seemed to be of the utmost importance to the science of medicine, as well as to the cause of science generally. Thus incited, I resolved to study such much-disputed subjects for myself, with the view of arriving at some independent opinion.

As the results of this work have tended to strengthen certain views concerning the epidemic and specific diseases, and to make plain some points which were previously involved in obscurity, I think I cannot do better than attempt a somewhat hasty review of facts, which seem to point conclusively to the necessity of entertaining opinions,

with respect to some of these diseases, which have been hitherto almost wholly ignored.

In the consideration of the nature and causes of disease, we have always to keep in mind two principal sets of factors. Each person exists with structural characters and functional properties which, though in the main similar to those of his fellow-men, have nevertheless individual peculiarities more or less marked. These may be inherent or acquired, habitual or occasional. Amongst these individual peculiarities are ranged what time-honoured custom has called the 'predisposing causes' of disease. On the other hand, man, with his individual peculiarities, lives in a world of change, exposed to the incidence of constantly varying external conditions, which, acting upon individual peculiarities, or upon the average human nature, become, in proportion to their deviation from the usual condition of things, so many 'exciting causes' of disease.

These two sets of factors must never be lost sight of. In the majority of cases, both come into operation in the production of the resultant morbid condition, although in others one or other of them alone may seem to be so potent as of itself to determine the morbid manifestation. The person who inherits a tendency to destructive lung disease may develop this morbid condition under the influence of exciting causes which would scarcely affect another who inherits no similar weakness (predisposing cause). On the other hand, just as contact with boiling water, owing to the exceeding potency of the 'exciting cause,' may determine a lesion of the skin in any individual (quite independently of the existence of a 'predisposition'), so may a person who is born with a weak and unstable nervous system become insane or epileptic, independently of the influence of any obvious exciting causes.

All diseases are, in fact, due to altered structure or molecular composition, whether visible or invisible, ascertainable

or non-ascertainable. They are no longer regarded as entities. They are due to changes of state in some portion of the body, whereby the vital movement in the part is diverted from a normal into an abnormal mode of activity.

The complicated structure of the human body, and the allocation of specific functions to specific parts, necessitates, and has occasioned, a functional correlation and interdependence. Any disturbance of this normal balance of functions of necessity entails a definite sequence of pathological states and actions. A morbid change in an important organ, if it interferes with the function of the part, rarely exists alone. It sets up other associated effects, whereby the disturbed equilibrium of functions is more or less replaced by a new adjustment.

The effects are often well marked, though very variable, when the disease is one in which any notable alteration in the composition of the blood occurs. Supplying the materials of growth for all parts of the body, any changes in the composition of the blood are found, now to affect one organ and now another most profoundly. Before entering, however, upon the consideration of those diseases in which changes in the nature and quality of the blood form the most important condition of the disease, it will be useful to dwell for a time upon some of the more local pathological changes that occur in the more solid tissues of the body. The two sets of phenomena are closely related to one another. Morbid states which are at first purely local may, after a time, produce general diseases; and a general or constitutional disease frequently entails limited lesions in special parts. A wound or a local inflammatory process may lead to thrombosis, gangrene, and blood-poisoning; just as, following a reverse order, various febrile conditions may cause local lesions—now in one organ and now in another.

There are so-called 'specific growths,' just as there are 'specific diseases' of a more general or constitutional character. The life-history of such growths as cancer and tubercle is a subject of great intrinsic interest; though the importance of their study is much enhanced by the fact, which I shall strive to make plain, that their modes of origin and distribution within the body are capable of throwing much light upon the origin and distribution of epidemic and specific infectious diseases amongst the community.

The term 'specific,' as applied to diseases, is confusing, and apt to carry with it a crowd of erroneous notions. Doctrines of 'specificity' have, however, been fashionable in medicine, though they are now growing more and more into disrepute. Thirty or forty years ago, amidst all the jargon concerning homoplastic and heteroplastic, euplastic and cacoplastic growths, would it not have been deemed rank heresy to profess a disbelief in the prevalent notions concerning the unalterable and 'specific' nature of cancer and of tubercle? Here were products altogether peculiar, and not derivable, as it was thought, from the normal tissues of the body—having laws of growth and distribution peculiar to themselves, and an origin which was shrouded in the mystery of a remote past. In view of this doctrine as to the specific and alien nature of the products, how natural was it that an undue stress should have been laid upon the fact that a tendency to the occurrence of such modes of growth may be hereditarily transmitted; how easily explicable is the facile and popular resort to the notion that, where multiple cancerous growths exist, the primary new formation has given rise to a seedling progeny by means of actual cancer 'germs.' Slowly but steadily these views have been undergoing a progressive modification. The anatomical elements of cancer and tubercle are now known to have no special and peculiar characteristics, and they are believed to be as easily derivable from

pre-existing tissues as are other non-specific morbid growths. A mere local change in the mode and intensity of pre-existing tissue-changes suffices to engender them. In the case of tubercle, this has been conclusively proved by such experiments as those of Dr. Burdon Sanderson and Dr. Wilson Fox. The latter says[1]: 'M. Villemin's position, that tubercle is a specific disease, producible by tubercle alone, cannot, I think, be held to be true; nor can the method of inoculation be used as a test of the tubercular character of any pathological product; for the four guinea-pigs in whom the vaccine lymph was inoculated, and those inoculated with putrid muscle, and even one beneath whose skin I simply inserted a piece of cotton-thread, and also one of the four in which, following Dr. Sanderson's example, I inserted a seton, presented as intense and typical specimens of the disease as those on whom inoculation had been practised with the most typical grey granulations from the lungs or the meningeal vessels.' What has now been (even experimentally) established with regard to tubercle, seems also to hold good for such 'malignant' growths as recurrent-fibroid, epithelial, and cancerous tumours or infiltrations. Statistics to which Virchow has drawn prominent attention seem most clearly to indicate the potency of 'exciting causes' in giving birth to these growths. Are they not found primarily, with by far the greatest frequency, in situations which are exposed to the action of irritative agencies, either external or internal, normal or abnormal? An amount of irritation which in some persons may lead to chronic inflammation or a hyperplasic overgrowth, will in others suffice to produce one of these so-called 'malignant' growths, even without the aid of any ascertained predisposition. The history of many cases of 'labial cancer,' and of that form to which chimney-sweeps are liable, explains almost as clearly the origin of

[1] 'On the Artificial Production of Tubercle,' 1868, p. 23.

cancer, as the results of experiments on the rodent animals explain one of the modes of origin of tubercle.

Is there anything specific in the mode of growth of these products, and in their subsequent distribution within the body of the affected person? Just as an erysipelatous inflammation spreads by gradually inducing a similar morbid action in adjacent parts, so does a cancer or a mass of tubercle grow by a slower extension of the morbid modes of growth. We have no more to do with a kind of implanted something, increasing by a multiplicative reproduction, in the one case than in the other. In both alike there are deviations from the ordinary modes of growth, which gradually extend to adjacent healthy parts. Neighbouring lymphatic glands become affected in the case of tubercle and cancer-growths, as well as where simple inflammations exist; and just as the change in the gland in the case of inflammation must be regarded as the result of a mere induced morbid action, rather than as the product of the multiplicative reproduction of a transmitted germ, so is a similar explanation open in the case of cancer and tubercle. Modes of growth which have been primarily induced may be also secondarily induced. The kind of agency, which is at least probably potential where the lymphatic system is concerned, or where particles of morbid growths come into contact with serous [1] or mucous surfaces, seems almost certainly operative when we come to consider that wider distribution which is occasionally brought about through the vascular system. The potency of the 'exciting causes' are here weakened, and new growths cannot be initiated in distant parts or organs by contact with disseminated particles, unless the 'predisposing causes' are favourable and there is an ability in the part to take on the morbid mode of growth. The action may be similar in kind

[1] See Dr. Sanderson's 'Report on the Communicability of Tubercle by Inoculation' ('Eleventh Report of Med. Officer of the Privy Council').

to that which the transplanted fragment of epidermis exerts upon the ulcerated surface. This becomes covered, not so much by an actual increase of the imported fragment as by the formative changes which its presence incites. A crystal thrown into a mixed solution of saline substances will also determine, by its mere presence, the crystallisation of similar materials from the solution; nay, it may determine, in addition, the crystallisation of other products whose modes of aggregation are more or less similar (isomorphous salts). The contact of any number of germinal particles with the tissues of an organ will not produce the formation of a new growth, unless the molecular actions (or modes of growth) existing in the part are such as to make the transition an easy one. The mere presence of 'germs,' therefore, is not all that is necessary. Cancerous masses may grow into the vena cava, and yet no cancer may spring up in the lungs: the stomach may be absolutely infiltrated with cancer, and yet, as I have recently seen, no similar growths may exist in the liver[1]. Detail, however, is needless on such a subject. The distribution of morbid growths throughout the body, as is well known, takes place, if at all, in a manner so irregular in different individuals, as to make the result wholly beyond the possibility of predication. Having to do with a case of syphilis, who would venture to fix upon the internal organs which would become affected? Who can account for all the irregularities observable in the generalisation of tubercle either in man or in the rodent animals? When cancer exists, who will affirm which organ shall be secondarily affected, and which not?

[1] We find, therefore, that in the absence of any apparent 'predisposition,' exciting causes, when potent, are sufficient to determine the occurrence of secondary growths, just as other exciting causes seem capable of determining the primary growth. As the exciting causes are weakened, the new formations occur only under the influence of a predisposition (hereditary or acquired), or of a natural aptitude on the part of the tissue to lapse into the morbid mode of growth.

The old notions as to the 'specific' nature of cancerous and tubercular products are supported, therefore, neither by the anatomical characters of the growths, by their mode of origin, nor by their mode of distribution; and the known facts concerning the hereditary transmission of a tendency to the formation of such products are certainly not more explicable in accordance with the old hypothesis than they are by the more modern view. Moreover, the history of these local so-called 'specific' growths, as others have in part indicated, will be found to throw much light upon the history of general so-called 'specific' affections, and their mode of distribution through communities, or from individual to individual[1].

Epidemic and acute specific diseases have many characters in common; they constitute a family the members of which are united by a certain bond of unity, though at the same time they are, in other respects, strikingly different from one another. The 'general' character of the symptoms originally gave rise to the notion that these affections were in the main dependent upon changes in the nature and quality of the blood. This view is still the one most commonly entertained, and the one which seems most likely to be true. But seeing that particular sets of symptoms recur with as much definiteness as individual differences of constitution will permit, we have a right to believe that the changes in the blood—however induced and of whatsoever nature they may be—are definite and peculiar for each of these diseases. The successive changes in the blood which are the immediate causes of the phenomena of small-pox, must be quite different from those giving rise to the morbid state known as typhoid-fever. Variable as these several groups of symptoms are amongst

[1] See Dr. Morris, 'On Germinal Matter and the Contact-Theory,' 1867.

themselves in individual cases, yet there is a general resemblance which suffices to maintain the distinctive nature of each affection. In this broad sense they are undoubtedly entitled to rank as 'specific' diseases. But though they may be presumed to be associated with definite changes in the blood, we have no right to infer that such changes of state can only be induced in one way. Many well-known chemical changes are capable of being brought about by more than one agency. And just as there is the best reason for believing that cancer or tubercle may be initiated *de novo* by the operation of irritants upon the tissues of certain individuals, and that such growths may subsequently be multiplied within the body by the contact-influence exerted by some of their disseminated particles; so may we suppose, not only that specific substances (contagia) may be capable of initiating specific changes in the blood, but that certain combinations of circumstances may, by their action upon the human body, entail similar definite changes and states of blood. Having to do with a perverted nutritive activity and mode of growth in a limited area of tissue, cancer or tubercle may make their appearance; whilst, having an altered nutritive activity and set of changes occurring in the blood, this all-pervading tissue may lapse into the successive states peculiar to one or other of the specific diseases, and so give rise to the symptoms by which they are characterized. This is by no means a forced analogy. Can cancer or tubercle arise in the individual without any pre-existing 'hereditary taint'? Can the states of blood peculiar to the several specific diseases arise *de novo*, or independently of contagion? These are questions whose import is really similar[1].

[1] This double mode of causation is perfectly familiar to the chemist. Particular chemical changes may occur under the influence of certain general determining conditions, which at other times (in the absence of

One of the great and distinguishing peculiarities of these specific diseases is their 'contagiousness.' Although very differently marked in the several affections, this property is as interesting as it is important. The fact of its existence seems always to have had a large share in determining the nature of the general views which have been held concerning these affections. Even in remote periods, by Hippocrates and others, they were commonly compared to processes of fermentation; whilst, since the time of Linnæus more especially, attention has been often prominently directed to the many apparent similarities existing between the commencement and spread of epidemic diseases, and the 'flight, settlement, and propagation of the insect-swarms which inflict blight upon vegetable life[1].' These analogies were seemingly strengthened by the increased knowledge which gradually arose concerning the various parasitic maladies to which man and the lower animals were liable. Writing in 1839, Sir Henry Holland says, in his essay 'On the Hypothesis of Insect-Life as a cause of Disease,' 'The question is, what weight we may attach to the opinion that

these conditions) may be even more easily initiated by a single specific cause. The introduction of a crystalline fragment into a saline solution, and its determination of the crystallisation of all the isomorphous salts contained in the solution, seems to be exactly comparable with the 'contagious' origin of diseases. But, under the influence of certain favouring conditions, crystallisation may occur without the contact of a crystalline fragment—the process may be 'spontaneous' in the same sense that the occurrence of the blood-change may be 'spontaneous.'

[1] Sir H. Holland's 'Medical Notes and Reflections,' second edition, 1840, p. 584. On the following page, the same author writes:—'Connected with these facts is the observation, seemingly well attested, that the cholera sometimes spreads in face of a prevailing wind, and where no obvious human communication is present—a circumstance difficult, if indeed possible, to be explained, without recourse to animal life as the cause of the phenomenon. No mere inorganic matter could be so transferred, nor is vegetable life better provided with means for overcoming this obstacle.' Whilst on the preceding page, the 'animal species' had been admitted to be 'minute, beyond the reach of all sense.'

certain diseases, and especially some of epidemic and contagious kind, are derived from minute forms of animal life, existing in the atmosphere under particular circumstances, and capable by application to the lining membranes, or other parts, of acting as a virus on the human body.' Now, the fact of the multiplication of the virus within the body was the peculiarity of these diseases which, above all others, caused such an hypothesis to be received with favour. Causes which are specific and which seem capable of self-multiplication—what can such agents be but living things of some kind, plant or animal? This mode of argument was, with many, all powerful. And when, after the discovery of the yeast-plant by Schwann, in 1836, new doctrines concerning fermentation began to prevail, the views of those who believed in the living nature of the specific causes of epidemic diseases were in part strengthened. If all fermentations were initiated by the agency of living organisms, and the specific diseases were comparable to processes of fermentation, then, how natural was it that many who were moreover influenced by the other analogies, should be led to imagine that the actual causes of these diseases were also living organisms? Only now, attention became directed to the much lower organisms which are so frequently associated with fermentative and putrefactive changes, instead of to insects 'minute beyond the reach of all sense.'

Here then is the origin of what in modern times has been termed 'The Germ-Theory of Disease.' Like homœopathy and phrenology, this theory carried with it a kind of simplicity and attractiveness, which insured its acceptability to the minds of many. Now, however, it seems to rest upon foundations only a little more worthy of consideration than those upon which these other theories are based. But, owing to its influence, in combination with the more generally received doctrines concerning the origin of life, there

has gradually grown up an unwillingness in the minds of many to believe that these contagious diseases can arise *de novo*. And, this being one of those beliefs which tends to curb inquiry, and to check the possible growth of sanitary knowledge in certain highly important directions, it seems to me necessary to look with scrutinising care to its foundations—not only with the view to the advancement of medical science, but with the direct object of removing all checks which may exist to the growth of sanitary precautions against the origin of these most pestilential affections.

Let us see, then, how far the 'germ-theory' fulfils the conditions which all good theories do fulfil—how far it explains a great number of the phenomena in question, without being irreconcileable with others.

The advocates of the 'germ-theory' have always rested their belief in it, in the main, because they considered that it offered a ready explanation of the increase of the virus of the contagious diseases within the body of the affected person. This increase, they suppose, is not otherwise to be explained. All other considerations brought forward in support of the theory are just as explicable by another supposition. Fully admitting that the occurrence of a process of organic self-reproduction would be a very adequate way of accounting for the increase of the infecting material, we must see whether this mere hypothesis can be reconciled with other characteristics of these affections. In the first place, it may be asked, whether such a process is actually known to constitute the essence of any general diseases. Because, if so, those in which it does occur, ought, in the event of the hypothesis being true, to present a close similarity to the diseases in which such a process is supposed to occur.

Now there are certain general diseases which do un-

doubtedly depend upon the presence and multiplication of organisms in the blood and throughout the tissues generally. There is the epidemic and highly contagious disease amongst cattle—known in this country by the name of the 'blood'—which excites in man that most dangerous morbid condition called 'malignant pustule.' The researches of M. Davaine[1], and others, have revealed the fact that this disease is essentially dependent upon the presence and multiplication of living organisms, closely allied to *Vibriones*, in the blood of the animals affected, and that similar organisms are also (locally) most abundant in the contagiously incited 'malignant pustule' of man. Unless this latter be destroyed in its early stages, the contained organisms spread throughout the body, and the disease speedily proves fatal. Of late, moreover, attention has also been called[2] to Pasteur's researches on the subject of the very fatal epidemic which raged for fifteen years amongst the silkworms of France. This affection, known by the name of *pébrine*, is dependent upon the presence and multiplication of peculiar corpuscular organisms, called *Psorospermiæ*, in all the tissues of the body. Both these general parasitic maladies are highly contagious; both are contagious by means of organisms; and in both the virus does increase by self-multiplication within the body of the animal affected. What more suggestive evidence could there be as to the truth of the 'germ-theory,' say its advocates, than is supplied by the phenomena of these two diseases? Undoubtedly the evidence is irrefragable as to its applicability to these particular diseases; but then comes the question, whether they are comparable with the other affections to which the germ-theory is sought to be applied? And this question must decidedly be

[1] See 'Comptus Rendus,' 1864 and 1865.
[2] 'Nature,' 1870, No. 36, p. 181.

answered in the negative. These parasitic diseases are sharply distinguished from the others by the fact of their almost invariable fatality. Creatures or persons once affected in this way are, under ordinary circumstances, thenceforth on the road to more or less immediate death. Happily, however, no fatality of this kind is characteristic of even such highly contagious diseases as scarlet fever and small-pox, or any other of the maladies with which parasitic organisms cannot be shown to be associated[1]. But if living things were really present as causes of these diseases, then most assuredly ought they to conform to that fatal type which is almost inseparable from the notion of a general parasitic disease, and which we find exemplified by the course of *pébrine*, the 'blood,' and 'malignant pustule[2].' Thus, the fact that the general tendency in the acute specific diseases is undoubtedly towards recovery rather than towards death, speaks strongly against the resemblance supposed to exist between them and the parasitic affections alluded to; and also against the hypothesis that they are dependent upon the presence of self-multiplying germs within the body. Such germs, when present and able to multiply in its blood, would be almost sure to increase until they brought about the death of their host.

These considerations alone should suffice to inspire grave doubts as to the truth of the 'germ-theory.' And such doubts may be reinforced by many others. Thus, the several affections being distinct from one another, this theory demands a belief in the existence of about twenty different kinds of organisms never known in their mature

[1] Doubtless there are other general parasitic diseases amongst animals. In almost all the specific diseases to which man is liable, however, I have invariably failed to discover any trace of organisms in the blood. The experience of many other observers has been similar to my own in this respect.

[2] See a paper by Dr. William Budd, in 'British Medical Journal,' 1863.

condition, but whose presence as invisible, non-developing germs is constantly postulated, solely on the ground of the occurrence of certain effects supposed to be otherwise incapable of occurring. That. if existent, they are no mere ordinary germs of known organisms is obvious, because their presence has again and again been shown to be incapable of producing the diseases in question. Mr. Forster says,[1] 'There is not perhaps on the face of the earth a human creature who lives on coarser fare, or to a civilised people more disgusting, than a Kalmuck Tartar. Raw putrid fish or the flesh of carrion—horses, oxen and camels—is the ordinary food of the Kalmucks, and they are more active and less susceptible of the inclemency of the weather than any race of men I have ever seen[2].' It has, moreover, been frequently demonstrated, that the organisms of ordinary putrefactions may be introduced even into the blood of man and animals without the production of any of these specific diseases[3]. Yet is the 'Antiseptic System' of

[1] See 'Med.-Chirurg. Rev.,' 1854, vol. xiii, where the supposed connection of diseases with processes of putrefaction is ably considered by the late Dr. W. Alison.

[2] The *Bacteria* which are sure to be abundant in such food cannot, therefore, be the much talked-of 'disease-germs.' Such a diet is, of course, by no means recommended, and could probably only be borne in certain climates by persons who lead a very active life. Epidemic diseases are frequently most fatal when they once break out amongst a people whose diet is of this kind. (See Dr. Carpenter, in 'Med.-Chirurg. Rev.,' 1852, vol. xi. p. 173.)

[3] See, amongst others, Davaine in 'Comptus Rendus,' Aug. 1864, and E. Semmer in Virchow's 'Archives,' 1870. Dr. Lionel Beale is well aware of this fact, and he, accordingly, whilst adhering to the 'germ-theory,' promulgates it under a new form. He says (' Monthly Microsc. Journ.,' Oct. 1870, p. 205):—'Concerning the conditions under which these germs are produced, and of the manner in which the *rapidly multiplying matter* acquires its new and marvellous specific powers, we have much to learn, but with vegetable organisms the germs have nothing to do. They have originated in man's organism. Man himself has imposed the conditions favourable to their development. Man alone is responsible for their origin. Human intelligence, energy, and self-sacrifice may succeed in extirpating them, and may discover the means of preventing the origin

treatment (good as it may be, irrespective of the germ-theory on which it has been based) pressed upon our attention on the assumption that the germs of putrefaction and the germs of disease are living organisms similar in nature. The strange persistency with which this view is advocated is not a little surprising when it entails the obvious contradiction that germs which do, under all ordinary circumstances, develop into well-known organic forms, should, when concerned in the production of the diseases in question, induce all the effects supposed to depend upon their prodigious growth and multiplication, and yet never develop, never become visible. And whilst *Bacteria*, and other organisms with which the unknown disease-germs are compared, flourish and reproduce in the much-vaunted germ-killing carbolised lotions [1], yet carbolic acid continues to be recommended solely on account of its germ-killing powers, and the theory on which the practice is based is thought to derive support from the results obtained by the use of this agent. Surely no theory could be weaker on which to base a successful method of treatment; and if, as its distinguished originator says [2], its general acceptance is principally hindered by the 'doubt of its fundamental principle,' then I would deliberately say that

of new forms not now in existence.' This is undoubtedly a very much less objectionable form of the 'germ-theory,' though much additional evidence would be needed before we could accept the view that contagious diseases are due to the rapid multiplication of the contagious particles within the body of the creature affected. The non-contagiousness of the blood (see next page) is as irreconcileable with this as with the other form of the 'germ-theory.'

[1] See Note I. p. xlvi. And in a recently published paper 'On the Relative Powers of Various Substances in Preventing the Generation of Animalcules or the Development of their Germs,' Dr. Dougall says, 'If, as is alleged, germs are the source of putrefaction, then the strongest preventives *must* be the best antiseptics, and *vice versâ*. Now, as seen in the table, carbolic acid occupies a very mediocre place as a preventive, therefore it is legitimate to conclude that it stands no higher as an antiseptic' (p. 13).

[2] 'British Medical Journal,' August 26, 1871, p. 225.

the blame, if any, cannot fairly be said to lie with those 'who have opposed the germ-theory of putrefaction.' The 'Antiseptic System' of treatment needs no support from a 'germ-theory'; it can be surely and unassailably based upon the broader physico-chemical doctrines of Liebig[1].

The last blow, however, seems given to the 'germ-theory' of disease, when we are told that the blood and the secretions in sheep-pox are not infective, though this disease is most closely allied to, and even more virulently contagious than, human small-pox. If germs had existed in this general disease, and their multiplication was the cause of it, then most assuredly would they have existed in the blood and in other fluids of the body; and yet, as Dr. Burdon Sanderson tells us[2], 'In sheep-pox all the diseased parts are infecting, while no result follows from the inoculation either of the blood or of any of the secretions; the liquid expressed from the pulmonary nodules has been found by M. Chauveau to be extremely virulent—certainly not less so than the juice obtained from the pustules.' Now, although in other of these diseases the blood does undoubtedly exhibit infective

[1] These doctrines do not seem to have been adequately grasped by Prof. Lister. Fragments of organic matter are believed by Liebig to be capable of acting as ferments; he, however, holds that their potency is deteroriated by heat almost as much as are the qualities of living ferments. The experiments with boiled fluids in bent-neck flasks, therefore, upon which Prof. Lister so strongly relies in proof of the germ-theory, prove absolutely nothing as between the two theories of fermentation of Liebig and of Pasteur. Amongst the atmospheric particles there are sure to be dead ferments, in the form of mere organic fragments. Now the doubt that previously existed was, as to whether they could initiate fermentation and putrefaction, or whether the presence of living germs was absolutely essential. In the experiments with bent-neck flasks, both fragments and germs must be simultaneously excluded or admitted to the fluids. Professor Lister's readers might suppose that Liebig had no objection to his ferments being boiled, and that the issue lay between the relative efficiency of oxygen and living germs. (See also Gerhardt's 'Chimie Organique,' t. iv. p. 545.)

[2] Report 'On the Intimate Pathology of Contagion,' in 'Twelfth Report of the Medical Officer of Privy Council.'

properties, still the ascertained existence of even one exceptional case amongst maladies so contagious as sheep-pox, seems to me absolutely irreconcileable with the truth of the 'germ-theory,' more especially when this theory was started principally to explain the phenomena of such highly contagious diseases [1].

In rejecting the 'germ-theory,' then, must we confess our absolute ignorance on the subject (a course always better than the adoption of an untenable theory), or are there facts to guide us to another view as to the nature and origin of the poisons of these infectious diseases?

It surely is a vice in argument to suppose that the increase of the virus within the body in these affections is only to be accounted for by a process of organic reproduction. The power of self-multiplication by division is peculiar to living things, but an actual increase of any substance may occur by a process of growth alone, without the aid of self-multiplication. Growth, however, takes place in not-living as well as in living matter; and, fundamentally considered, it only means increase in the quantity of the substance which grows, whether we have to do with the substance of a muscle, with a crystal, or with a complex organic poison. Liebig says: 'A substance in the act of decomposition, added to a mixed fluid in which its constituents are contained, can reproduce itself in that fluid.' And in illustration Sir Thomas Watson writes: 'Thus the virus of small-pox (which virus is formed out of the blood) causes such a change within the blood as gives rise to the reproduction of the poison from certain constituents of that fluid: and whilst the process is going on

[1] Inoculation with the blood of a person suffering from measles has in several cases failed to reproduce the disease. The different severity of small-pox taken in the ordinary way, and that induced by 'inoculation' of the matter of a small-pox pustule, is also quite inexplicable in accordance with the 'germ-theory,' although both facts are quite reconcileable with the view about to be mentioned.

the natural working of the animal economy is disturbed; the person is ill. The transformation is not arrested until the whole of that ingredient in the blood which is susceptible of the decomposition has undergone the metamorphosis[1].' The specific poison (contagium) does not, however, seem to be immediately reproduced in the blood of the person affected: rather, a set of changes are set up in the blood which ultimately lead to the evolution of such a poison in some part or parts of the body; so that, as Mr. Simon says[2], 'Bowels, skin, kidney, tonsils, are the favourite resorts of the several fever-poisons just as they are the surfaces by which naturally the organic waste of the several tissues is eliminated[3].'

There are many organic poisons which undoubtedly produce spreading changes in the blood. Writing from Australia, Prof. Halford says[4]:—'In fatal cases of snake-poisoning, whether in this colony, India, America, or Africa, it may be stated as a rule, with few exceptions, that the blood loses its

[1] Ch. Robin says. in his 'Végétaux Parasites,' 1853, p. 376:—'On a confondu un phénomène grossier et physique, de transport de végétal d'un sol sur un autre, plus ou moins favorable, avec la question de contagion. Celle-ci est au contraire caractérisée par une modification moléculaire lente des substances organiques se propageant de proche en proche, sous l'influence du contact d'autres substances organiques présentant déjà elles-mêmes une modification analogue. S'il y a quelque chose de contagieux dans cette transplantation, c'est la putrefaction des substances azotées qu'on transporte, et elles déterminent dans les mucus sains un altération analogue à celle qu'elles ont éprouvée. Mais il n'y a rien là qui appartienne en propre au végétal et doive lui être attribué.' See also pp. 307–309.

[2] Lectures on Pathology.

[3] A similar view has been advocated on more than one occasion by Dr. B. W. Richardson. He says ('Medical Times and Gazette,' November 5th, 1870, p. 539):—'A person suffering from a communicable disease is poisonous precisely as a cobra di capello is poisonous—that is to say, he is producing by secretion an organic poison, which, if it comes into contact in the right way with a healthy person, will reproduce disease.' See also the 'Transactions of the Epidemiological Society,' vol. i.

[4] On the Treatment of Snake-bite. 1870.

power of coagulation and becomes thinner and poorer.' After the death of the person 'it greedily absorbs oxygen when exposed to the air, and it absorbs it more than unpoisoned blood.' Though the precise changes are quite unknown, its constitution is obviously profoundly modified[1]. The rapidity with which the symptoms are produced in the case of snake-bite do not in the least prevent our comparing the effects of snake-poison[2] with those of the contagious zymotic diseases. In some of these the effects have been even more rapidly produced. Speaking of 'the Black Death,' which raged in the fifteenth century, Hecker tells us that, 'Many were struck as if by lightning, and died upon the spot, and this more frequently among the young and strong than the old.' Again, Dr. Aitken says: 'When the cholera reached Muscat, instances are given in which only ten minutes elapsed from the first apparent seizure before life was extinct'; whilst instances of death taking place from cholera-poison in two, three, or more hours, are well known to be extremely common.

> 'Its effect
> Holds such an enmity with blood of man
> That, swift as quicksilver, it courses through
> The natural gates and alleys of the body.'

The action of known poisons, whether animal or other,

[1] Dr. Richardson has ascertained that, unlike vaccine lymph, the snake-poison becomes weakened by dilution; and similar observations have been made by others. The 'particulate' nature of the poison in vaccine lymph, which has been demonstrated by the skilful experiments of Chauveau and Sanderson, is a condition in which it very probably exists in many other contagia.

[2] That such effects are in no way necessarily dependent upon the fact that this poison contains living elements, we may imagine from the influence of prussic acid, morphia, etc. Nay, more; I have had frequent personal experience of the fact that a spasmodic and catarrhal affection somewhat resembling hay-fever may be produced by emanations from certain Nematoid worms, even after they had been preserved for two or three years in spirits of wine, and macerated for a time in calcic

upon the blood and system generally, may therefore be compared with those unknown poisons of the zymotic diseases. The great difference is this. The changes in the blood induced by snake-poison are not such as to terminate in the elaboration of a similar poison in any part of the body of the person bitten, whilst the bite of a mad dog does lead to changes which culminate in the reproduction of the hydrophobic poison; and similarly with those of scarlet-fever or small-pox—contact with these poisons entails changes which result in an enormous production of similar poisons. There is probably no fundamental difference between the two sets of cases. The malarial *miasm* of intermittent fever, and the poisonous state of the blood which leads to the production of rheumatic fever [1], as a rule produce effects which are more strictly comparable with those of snake-poison, though there is reason to believe that these diseases may merge into other affections which are admitted to be contagious—as when intermittent or remittent fevers develop in warm climates under the aggravated form of contagious yellow fever. In this way may the gulf be bridged which seems to separate the effects of snake-bite from those of hydrophobia. As Liebig pointed out, what occurs in the former case may be compared to the action of yeast upon a simple solution of sugar, and in the latter to the action of the same ferment upon a solution of sugar which also contains nitrogenous

chloride (see 'Philosoph. Transact.' 1866, p. 583). Effects somewhat similar, though not so lasting, are produced upon some persons by the smell of powdered ipecacuanha.

[1] I agree with Dr. Richardson in thinking that this affection really belongs to the zymotic class of diseases. Dengue seems to be a slightly contagious affection somewhat intermediate between rheumatic and scarlet fever. The 'sweating sickness' of the middle ages was considered to be an aggravated epidemic form of rheumatic fever, and so also with the various forms of 'miliary fever.' The contagiousness of these diseases, according to Hecker, seemed to vary in different epidemics.

materials—at the expense of which the ferment is enabled to grow. Thus, then, just as the presence of a crystalline fragment may determine the synthesis of its elements from a solution in which they are contained[1], and as the living ferment may bring about that much more complex synthesis which occurs during its growth, so may an organic poison (having an intermediate molecular complexity) by its contact with the fluids or mucous surfaces of the body, be enabled to determine a series of changes leading to the synthesis of a similar poison[2].

If we find that amongst this class of general or specific diseases some are not at all, and others only slightly, contagious, whilst the remainder present increasing degrees of contagiousness; that diseases, which sometimes or under some conditions are non-contagious, under others become contagious; and lastly, if we find that even the virulently contagious poisons of some diseases are undoubtedly capable of arising *de novo*, then have we certain reasons for the supposition, that the contagiousness or non-contagiousness of particular general diseases is a quasi-acci-

[1] These elements, as Prof. Graham showed, really exist separately in the solution, since they are separable by dialysis.

[2] Sir Thomas Watson says, in explanation of Liebig's doctrine, 'In order, then, that a specific animal poison should effect its own reproduction in the blood, and excite that commotion in the system which results from the formation and expulsion of the new virus, it is requisite that a certain ingredient (analogous to the gluten in the brewer's wort) should be present in the blood, and this ingredient must have a definite relation to the given poison.' And he subsequently adds ('Principles and Practice of Physic,' vol. ii. p. 790):—'This theory of Liebig's offers, then, an intelligible explanation of the curious facts that certain contagious disorders furnish a protection, temporary or permanent, against their own return; that they have a tolerably definite period of incubation, and run, for the most part, a definite course; that some persons are less susceptible than others of the influence of these animal poisons, or not susceptible at all; and that the same individual may be capable of taking a contagious disease at one time, and not at another.' The same facts, it may be observed, are almost inexplicable in accordance with any rational rendering of the 'germ-theory.'

dental feature, and that there is no real difference in kind between the poison of a serpent which does not occasion the production of a similar venom, and the poison of a mad dog which does lead, directly or indirectly, to the re-evolution of a similar virus[1].

Let us take a brief survey of some of the facts which are known concerning these specific infective diseases.

Glanders is an affection which is in many respects analogous to syphilis, and is almost, if not quite, as highly contagious a malady. Both these diseases, too, form extremely interesting links between such specific tissue affections as cancer and tubercle, and such infective blood-diseases as small-pox and scarlet-fever. Like the former, they are apt to involve the presence of morbid growths scattered in different parts of the body, though, like the latter, they are commonly spread by contagion from individual to individual. However little we may know concerning the actual origin of syphilis, no doubt seems to remain in the minds of most of those who have studied the question, as to the possibility of producing glanders in the horse. After referring to the highly contagious nature of the affection, Dr. Gavin Milroy

[1] In snake-bite the symptoms are due to the effects of an habitually poisonous secretion which has a most rapid and deadly action; whilst hydrophobia is due to the effects of an occasional quality of the salivary secretion. This occasional quality, characteristic of rabies, is generally admitted to arise independently in the dog, and yet the poisonous salivary secretion sets up a similar disease in other dogs which may be bitten. Nay, more, this affection at times prevails in an epidemic fashion. Dr. Gavin Milroy says ('Transactions of the Epidemiological Society,' vol. i. p. 173): 'Hillary, in his work on *Barbadoes*, described *rabies* as common in the West Indies. Moseley, having never seen a case of it for a series of years, doubted the correctness of the statement; but, in 1783, it unexpectedly broke out with violence at Hispaniola, and also in Jamaica, where it prevailed from June to the following March. Dogs were seized with it that had no communication with others, and some dogs not brought on shore went mad in the harbours of the island. "On Tropical Diseases," 1803.' There are those, however, who still doubt whether rabies is capable of arising *de novo*. (See Art. in Reynolds's 'System of Medicine,' vol. i.)

says, on this subject: 'It is also very generally admitted that glanders is a generable as well as a propagable disease; and that it is extremely apt, especially in some seasons, to develop itself in foul, unventilated stables, or (as was often the case during the continental war) in the filthy between-decks of crowded transports[1].'

Here too may be mentioned such affections as purulent ophthalmia, gonorrhœa, croup, and diphtheria—the two former at least yielding local secretions which are virulently contagious, although assuredly they are not necessarily produced by specific infecting agents. The secretions of croup are only slightly contagious, though those of diphtheria often exhibit this quality to a more marked degree. Yet, even this last is generally regarded as an aggravated form of angina, which is apt to prevail occasionally as an epidemic affection[2].

Turning now to the infective diseases of a more general

[1] 'Transactions of the Epidemiological Society,' vol. i. p. 175. The same author adds, however: 'The converse of the proposition is happily no less true; experience having abundantly shown that its development may be controlled even to absolute prevention by the same simple sanitary rules, the observance of which has banished from our jails and workhouses the disease to which I shall next refer, viz. typhus.'

[2] After referring to the exaggerated notions which were at one time entertained with regard to the contagiousness of diphtheria, Mr. J. Netten Radcliffe ('Trans. of the Epidem. Soc.,' vol. i. p. 332) says:—'Subsequent observation has shown, moreover, that contagion plays but a very limited part in the epidemic extension of diphtheria. . . . The times of occurrence of the forerunners of the epidemic, the scattered and disconnected centres of manifestation, and the slow growth, extending over a period of several years, would seem to point to developing causes, slowly originating over the whole or the greater portion of the surface of the kingdom, and culminating more rapidly in the southern than in the northern districts.' Mr Radcliffe adds, 'If we would successfully study the etiology of the epidemic, we cannot disconnect that study from the observation of allied affections prevailing contemporaneously.' An examination of the statistics relating to the prevalence, during the same period of scarlet fever, croup, thrush, quinsey, and laryngitis lead to the conclusion that '*all the affections allied to diphtheria prevailed epidemically contemporaneously with diphtheria.*'

character, we find a group of the utmost importance to the surgeon and to the obstetrician—between the members of which there is the closest alliance and even interchangeability—and concerning the possibility of whose *de novo* origin no surgeon or physician can entertain any reasonable doubt. These are erysipelas, puerperal fever, pyæmia, and hospital gangrene—fearful affections, and all only too easily producible[1]. Not to mention idiopathic erysipelas, which is also a contagious affection[2], how frequently does an ordinary inflammation assume an erysipelatous character in certain individuals—more especially in those who are the subjects of renal disease: and yet hospital gangrene, pyæmia, and puerperal fever, are but different modes in which this morbid process repeats itself in certain constitutions and under certain conditions. How easily erysipelas is set up in some persons by the mere contact of a wounded surface with the fluids of a dead body, is well known; and how fatal and frequent may be the attacks of puerperal fever due to the same cause has been fully established by melancholy experience at the Vienna Lying-in Hospital. Yet that such effects are in no way attributable to, or comparable with, ordinary processes of putrefaction is also a matter of absolute certainty. Again, in certain cases where symptoms of poisoning result from eating mackerel or some shellfish, we know that these effects are not due to the putrescence of such articles of food. And similarly, in reference to the many cases in which symptoms of poisoning have been produced in Germany by sausages, we learn from Liebig that 'the sausages are only poisonous *at a particular*

[1] Sir William Jenner says ('Practical Medicine of To-Day,' 1869, p. 56):—'We know that the zymotic element which produces contagious pyæmia may be generated in the frame of man *de novo*. A most important problem to be solved is that of the spontaneous origin of other zymotic diseases.'

[2] Sir Thomas Watson's 'Practice of Physic,' vol. ii. p. 917.

stage of decay, and cease to be so when putrefaction is advanced so far that sulphuretted hydrogen is evolved; the central part being often poisonous whilst the surface is 'wholesome.' There seems every reason to believe that in the changes (short of actual putrefaction) which may take place in these substances, a 'peculiar poisonous principle is evolved.' And so in certain cases where an *unhealthy process* of suppuration occurs, poisonous products may be generated in a wound, whose absorption into the system is capable of bringing about those general symptoms of blood-poisoning which are characteristic of puerperal fever or of pyæmia [1].

If we refer now to the diseases which are most frequently endemic or epidemic in nature, we find them presenting very different degrees of contagiousness. The communicability of some of these affections seems to vary in different epidemics, and also, even during the same epidemic, in different places. Independently of this individual variability, however, the diseases, looked at as a series, present remarkably different degrees of contagiousness. In some this property seems to be absent, whilst in others it presents a most sure and deadly virulence.

Ordinary intermittent and remittent fevers are, like rheumatic fever, endemic rather than epidemic, and may, as we know only too well, be developed in almost any individual (and especially in a new comer) when he ventures into a malarious district. All attempts to connect malaria with the presence

[1] Just as contact with particular compounds (e.g. cadaveric poison) seems to favour the production of such poisonous compounds in a wound, so may the presence of carbolic acid tend to hinder those poison-generating changes which are otherwise apt to occur in some wounds. The success which attends the use of carbolic acid may, therefore, be quite independent of its germ-killing powers, or even of its ability to arrest putrefactive processes in general. It has been shown, indeed, to act quite differently with different fermentable fluids. ('Modes of Origin of Lowest Organisms,' 1871, pp. 81–85, and Dr. Dougal's pamphlet, p. 6.)

of organisms have signally failed; these fevers, indeed, prevail in the most variable sites, and are by no means restricted to marshy districts. Dr. Fergusson says: 'The first time I saw intermittent and remittent fever become epidemic in an army was in 1794, when, after a very dry and hot summer, our troops, in the month of August, took up an encampment at Roosendaal in South Holland. The soil was a level plain of sand with perfectly dry surface, where no vegetation existed or *could* exist, but stunted heath-plants. On digging, it was universally found percolated with water to within a few inches of the surface, which, so far from being at all putrid, was perfectly potable in all the wells of the camp.' These diseases, under ordinary circumstances, are most certainly not contagious, and yet all the best authorities on the subject are agreed that yellow fever, which is capable of being propagated by contagion *under circumstances favourable to its extension*, is but an aggravated form of remittent fever, as it occurs in warm countries[1]. This gradual conversion of a non-contagious into a contagious form of disease, combined with the limitations as to the nature and degree of its contagiousness, which the widest experience compels us to accept, are facts of the utmost importance for those who seek to learn the nature and origin of the contagious influence. And, as almost similar limitations have to be accepted with regard to the contagiousness of cholera and dysentery, it is of the greatest importance to ascertain the nature of these

[1] 'On Marsh Fever,' in 'Edinburgh Philosophical Transactions,' vol. ix. p. 274. And yet, concerning this disease, Dr. Milroy says:—'That yellow fever is constantly making its appearance, at intervals more or less distant, in various tropical countries, quite independently of any suspicion of antecedent importation, just as malignant cholera does in Hindostan, does not admit of doubt. In some seasons, from causes which we have hitherto failed to discover, it exhibits a much greater diffusion and migratory power than in other seasons. . . . Malignant cholera is much more diffusible and migratory than yellow fever; few regions of the world have escaped its assault.'

limitations. Facts abound, and speak most plainly to those who will read them dispassionately. Referring to the prevalence of yellow fever on the coast of Brazil, Dr. McKinlay[1] wrote:—'Almost every person who joined the *Vestal* during the prevalence of fever was affected by it; but no person leaving her, under the disease, communicated it to another, in another place.' That is, as he afterwards explained, so long as the affected persons went to a healthy place in which the disease was not prevailing.

Occurrences of this kind are most notorious; and, when an epidemic of yellow fever occurs on land, it has often been found that there are boundaries at no great distance from the tainted district where the disease has not, and to which it will not, spread[2]. The value of migration from the affected region is a matter of history, and the circumstances which have revealed it have all the value of experiments conducted upon a large scale. 'During the epidemic of 1800, at Cadiz, 14,000 persons left that city when the disease became suspected. These people fled to the country, where they remained free from the epidemic; while of the 57,499 who remained, 48,520 were attacked, of whom 6,884 lost their lives.' And again we read[3]:—'It was calculated that from Barcelona, in 1821, about 80,000 persons fled; and, except some who departed with the disease already upon them, or who were on the eve of being attacked, all remained exempt from the reigning malady.' But when individuals from an infected district pass into a region where conditions prevail which are favourable to its spread, or which are themselves capable of engendering typhus or other fevers, then yellow fever appears to be a contagious disease. A good

[1] 'Monthly Journal of Medical Science,' November, 1852, p. 425.
[2] See 'Med.-Chir. Rev.,' 1851, vol. xiii. p. 338.
[3] 'Second Report on Quarantine,' etc., p. 202.

illustration of this is supplied by Sir Gilbert Blane [1]. He says:—'On the 16th of May, 1795, the *Thetis* and *Hussar* frigates captured two French armed ships from Guadaloupe, on the coast of America. One of these had the yellow fever on board; and out of fourteen men sent from the *Hussar* to take care of her, nine died of this fever before she reached Halifax, on the 28th of the same month, and the five others were sent to the hospital sick of the same distemper.' So far, there is nothing whatever unusual; but what follows is a good example of the kind of testimony which exists as to the occasional contagiousness of the disease. 'Part of the prisoners,' we are told, 'were removed on board the *Hussar*, and, though care was taken to select those seemingly in perfect health, the disease *spread rapidly in that ship* (formerly healthy) [2], so that near one-third of the whole crew was more or less affected by it.' Now, these facts which are recorded concerning yellow fever, are very comparable with what would have to be stated concerning dysentery. This also is 'a disease liable to be engendered at any time by foul, damp air, and the use of bad food and drink, and which, at first, shows little, if any, power of communicability, but which, as cases multiply, and when the sick and the well are congregated together, unquestionably acquires contagious properties [3].' The same power of arising *de novo*, and the same absence of contagiousness, except under the influence of favouring circumstances, seem to distinguish the direst of our modern epidemics—cholera. As Dr. Gavin Milroy says: 'The whole history of the disease proves that contagion plays a very small and subordinate part in its diffusion; and nowhere has the attempt to exclude it by barring intercourse

[1] 'Diseases of Seamen,' p. 606.
[2] That is, free from yellow fever.
[3] Dr. Gavin Milroy, loc. cit., p. 176.

with places already affected succeeded in protecting a country from its invasion.' Out of the area in which it habitually exists as an endemic disease, malignant cholera does not seem to be directly generable 'by any known or appreciable conditions of local insalubrity, however much these conditions may favour its development or aggravate its intensity when it is once present, or is close at hand.' The spread of the disease from its endemic site seems undoubtedly to be influenced by obscure atmospheric or other unknown conditions, comprised under the term 'epidemic influence.' Sir William Jenner asks: 'What is the specific cause-relation between cholera and choleraic diarrhœa, and between severe summer diarrhœa and choleraic diarrhœa? Is cholera, in the form of choleraic diarrhœa, always amongst us?' And Mr. Macnamara, in part, replies from Calcutta that 'cholerine is simply a modified form of Asiatic cholera, and is capable of engendering this more deadly form of the disease in other people by means of the dejecta.' He says, also: 'I know that several of the leading practitioners in this part of India are of opinion that cholera is "a something generated in the bodies of those attacked by it, quite independently of all external influences[1]."'

Turning now to such affections as influenza and mumps, these also are diseases which present various degrees of contagiousness, and are frequently epidemic in their mode of onset. Both are believed to be capable of arising *de novo*[2],

[1] 'A Treatise on Asiatic Cholera,' 1870, p. 327. It is only fair to add, however, that Mr. Macnamara does not give his assent to this view. He is a firm believer in the communicability of cholera. He admits that 'sporadic cholera' is easily generable *de novo*, and that 'cholerine,' from which it is often quite indistinguishable, is capable of giving rise in others to malignant cholera; and yet he wishes to maintain the distinctiveness of the latter form of the disease. But other affections also exhibit different degrees of contagiousness, and it would seem to us that 'sporadic cholera,' which is easily generable in certain parts of India, cannot really be distinct from 'cholerine.'

[2] Dr. Morris says ('Germinal Matter and the Contact-Theory,' 1867,

although the spread of influenza is undoubtedly promoted by unknown 'epidemic influences.' Sir Thomas Watson says: 'The visitation is a great deal too sudden and too widely spread to be capable of explanation' by mere contagion. He adds: 'It has been observed to occur also at the same time on land, and on board different ships, which have had no communication with the shore nor with each other[1].'

If, however, we direct our attention to such affections as typhoid fever, relapsing fever, typhus, the plague, and cerebro-spinal meningitis, we meet with a group in which different degrees of contagiousness are presented, but concerning the origin of which *de novo*, or independently of contagion, there can now be little doubt. Although this is a doctrine which has long been supported by many who have paid most attention to these diseases, it has been much enforced and strengthened, of late years, by the investigations of Dr. Murchison. The contagiousness of typhoid or enteric fever is very low; and, as Dr. Murchison says, 'although enteric fever is, under certain circumstances, communicable, a large number of cases commence under circumstances which appear to exclude every possible source of contagion. The truth of this observation is almost universally admitted; and it is, therefore, necessary to search for some other cause of the disease than contagion.' An enormous amount of evidence tends to show

p. 70):—'A curious contagious disease is recorded by Huxley to have arisen on board the surveying vessel *Rattlesnake*, characterised by glandular and diffuse cellular inflammation, by common and phlegmonous erysipelas, and by *mumps*.'

[1] 'Principles and Practice of Physic,' vol. ii. p 43; where examples are given. On this subject, also, Dr. Gavin Milroy says: 'It has been confidently stated that every known visitation of the epidemic in the Faroë Islands has been preceded by the arrival of a vessel or vessels from Denmark, when it was prevailing there. But such a statement must not be too readily received; as it is well known that other islands, equally distant from any continent, have been visited, quite independently of arrivals therefrom.'—(See also the article on 'Influenza' by Dr. Parkes in Reynolds's 'System of Medicine,' vol. i.)

that emanations from sewage and from *some forms* of putrefying matter are capable of exciting the disease in those who are favourably predisposed, although, in other cases, it seems to be more directly communicated by means of drinking water contaminated by sewage containing the dejections from a typhoid patient[1].

Relapsing fever and typhus present many points of resemblance: both are essentially epidemic diseases; both are undoubtedly contagious. They generally occur during seasons of great scarcity, and they prevail most widely amongst the poorest class of the population. Overcrowding and defective ventilation, especially when associated with bad and insufficient food, 'not only favour the propagation of typhus, by concentrating the emanations from the sick, but

[1] Referring to the views of Dr. W. Budd and others as to the disease being propagated only by sewage which is contaminated by typhoid stools, Dr. Murchison says:—'Admitting fully that this view offers the best explanation of those cases where the fever is propagated by the sick, many, if not most, of the facts adduced in its support are explicable on the theory of spontaneous generation, while in the others the mode of transmission is less clearly established than might be desired. On the one hand, facts are adduced to show that the disease is contagious; and, on the other, cases are mentioned to demonstrate the intimate connection between its origin and bad drainage. The evidence, however, is still insufficient to prove that the stools of the sick have constituted the medium of communication. This conclusion, it seems to me, has been jumped at from the unwillingness to admit that a communicable disease can ever have a spontaneous origin. But, in the second place, there are many facts which show that enteric fever often arises from bad drainage, independently of any transmission from the sick. As long as the current flows freely through a drain, there is little danger of the emanations from it giving rise to enteric fever. The danger arises when the drain becomes *choked up*, when the sewage stagnates and ferments.' The dejections from a typhoid patient being remarkably prone to undergo decomposition, Dr. Murchison adds: 'It is possible that the stools of enteric fever are more prone than ordinary sewage to undergo the *peculiar* fermentation by which the poison is produced, and that even in certain cases the fermentation may have commenced before their discharge from the bowels. In this way, enteric fever may occasionally be propagated by the stools, but even then it seems more probable that the poison is always the result of *decomposition*, than that it is derivable from a specific eruption like that of small-pox.' ('On the Causes of Continued Fevers,' in 'Lond. Med. Rev.,' 1863).

appear to be capable of generating the poison *de novo.*' After alluding to the mode in which epidemics commence, Dr. Murchison adds: 'I would allude in particular to an epidemic of true typhus which occurred in 1843, at Broulhac, an elevated village in the Canton de Puy, in France. Most of the inhabitants were in a state bordering on starvation; and the first cases were traced to a house where there was overcrowding and no ventilation. It is impossible to conceive that the disease was imported, inasmuch as true typhus was not prevalent at the time in any other part of France[1].' With regard to relapsing fever, on the other hand, it has been shown[2] that this (which is essentially *the* famine fever) is more dependent upon extreme starvation than upon overcrowding. Although it is not always easy to separate these two causes, it has been ascertained that in mixed epidemics of typhus and relapsing fever, relapsing fever is most prevalent towards the commencement, and typhus towards the close, of the outbreak. Then, again, we know that relapsing fever is not confined to large towns, but that it also decimates the starving inhabitants of country places.

[1] Dr. Murchison very aptly remarks:—'It has been the custom with many writers to refer epidemics of typhus to some subtle "epidemic influence;" and thus when a failure of the crops has been followed by typhus, both of these disasters have been ascribed to a common atmospheric cause. But of such atmospheric influences, capable of producing typhus, we know nothing; their very existence is doubtful, and the employment of the term has too often had the effect of cloaking human ignorance, or of stifling the search after truth. If typhus be due to any "epidemic influence," why does this influence select large towns and spare the country districts? Why does it fall upon large towns in exact proportion to the degree of privation and overcrowding among the poor?' (loc. cit.) Still, although the prevalence of typhus fever may be in great part accounted for without resorting to unknown 'epidemic influences,' it must not be supposed that there are no unknown cosmical influences which have to do with the outbreak and spread of various epidemic diseases. Let us rather admit that which seems so probable, and live in the hope that we may one day ascertain more concerning their nature.

[2] See Dr. Murchison's 'Continued Fevers of Great Britain.'

Cerebro-spinal meningitis is believed by many to be only a modified form of typhus [1], though this is more certainly the case with the plague, in which the typhus poison is evolved in its severest form. Undoubtedly contagious, though formerly believed to be infectious, in the very highest degree, Dr. Gavin Milroy says: 'The whole history of medical opinion on the subject of the plague affords one of the most remarkable instances on record of fanciful speculation taking the place of sober and careful inquiry.' And then he adds: 'That the plague has frequently become developed *de novo*, and quite independently of any antecedent infection, cannot be doubted. The recent outbreak at Bengazi, on the Barbary coast, only confirms previous testimony; and as this outbreak occurred after many years' disappearance of the pestilence in that place, as well as throughout Egypt and Turkey generally, no other interpretation is possible. Then, as on many other occasions, the disease sprung up amongst want, wretchedness, and squalor, and its true nature was not recognised for many weeks, in consequence of its close resemblance to ordinary typhus, to which it seems to be nearly allied [2].'

Now the remaining members of the group of specific infective diseases are varicella, hooping-cough, measles, scarlet fever, and small-pox. The knowledge which we possess concerning the mode of origin of these otherwise than by infection, is almost *nil*. They differ amongst themselves, it is true, as regards their degree of infectiousness; but, as

[1] Doubts, however, are entertained on this subject. (See Mr. Radcliffe's article in Reynolds's 'System of Medicine,' vol. ii.)

[2] 'Transactions of the Epidemiological Society,' vol. v. p. 174. This is generally the rule with regard to epidemics. They occur mostly at times when other ordinary or non-specific affections, to which they are most closely related, are prevalent. And during the period of their decline, the more virulent epidemic forms of the affection again seem to lapse into more ordinary and non-contagious forms of disease.

others have suggested, they are probably more strictly dependent upon individual states than upon external conditions, and consequently are more baffling to those who attempt to fathom their causes. Measles, scarlet fever, and small-pox, are undoubtedly amongst the most contagious of diseases, and therefore the chances are always strongly in favour of their contagious origin in any given case. But should this satisfy us? Should we be content to say that even measles, scarlet fever, and small-pox, are propagable only by means of contagion, and cannot arise *de novo*[1]? Are they not strictly comparable with many other general infectious diseases which undoubtedly arise 'spontaneously'? Do we not see amongst those which may so arise that the degree of contagiousness is altogether variable? Does not this seem to increase gradually in each affection, as the off-cast particles tend to undergo molecular changes, which are more and more capable of initiating chemical actions of a spreading character in the blood, or mucous

[1] It seems to me that at present the facts are looked at much too exclusively from one point of view. It is fully admitted by many persons that during epidemics, more especially, a large number of cases of small-pox occur, even in isolated situations, in which it is quite impossible to obtain any evidence of contagion. When we consider further that the disease is epidemic at times, and then almost dies out, although multitudes remain who might be infected, we must admit that something besides contagion is undoubtedly operative in facilitating its spread during these times, and therefore we may assume it to be possible that this 'epidemic influence' of itself might, in certain persons, suffice to engender the disease without contagion. Dr. Gavin Milroy (loc. cit.) says: 'This most interesting subject has not been investigated with that patient and searching care which all physical problems demand. The prevailing negative belief rests on merely presumptive grounds, rather than on sifting inquiry. That outbreaks of measles, hooping-cough, etc., have been observed in various remote islands, and at distant intervals of time, without any traceable connexion with previous cases, either in the country itself or amongst recent arrivals, can scarcely be doubted. Hillary particularly alludes to his having noticed such occurrences in Barbadoes; and the medical history of other West India islands would afford, I believe, similar evidence.' (See also Hecker's 'Epidemics,' pp. 215-218.)

surfaces of ordinary individuals? And does not the diminishing contagiousness of different diseases seem to be due to the fact that off-cast particles in these affections are less and less capable of acting upon the healthy fluids and mucous surfaces of the body, but require them to be altered, now by one set of agencies affecting the general health and now by another, before any of such particles can initiate those changes which lead to the evolution of similar specific poisons within the body? Hooping-cough, measles, scarlet fever, and small-pox, would, in this case, be merely the last terms of a series, differing from the other members simply in degree, but not in kind—and therefore as capable of being generated *de novo* as either of the others, although much more capable than they are of being disseminated by means of contagion.

If we reject this notion, what remains for us? The germ-theory is quite untenable—the analogy which has been thought to exist between the causes and nature of certain diseases and the specific and unalterable characters of living organisms is erroneous in both its aspects. And even if the diseases are *now* only propagable by contagion—just as the higher living things are propagable by reproduction—they must nevertheless have originated once; and, if once, why not now? Or, declining to admit even so much, shall we refuse to bear our own burdens? Shall we shift the difficulty, and suppose that the poisons of syphilis, measles, scarlet fever, small-pox, and other diseases, have been evolved amidst the unknown conditions obtaining upon the surface of an unknown world, whose disruption has scattered them broadcast, and conveyed them to us, with other never-dying germs, upon the verdant surface of a 'moss-grown fragment'? With such alternatives, surely our choice cannot be doubtful.

If we turn to a sober survey of the facts which lie before

us concerning the infective diseases as a class, our difficulties will be much diminished: simple and obvious conclusions will appear.

COMMUNICABLE DISEASES.

PARASITIC DISEASES AFFECTING—

Many of them capable of arising de novo.

- External (cutaneous) surface.
- Internal (mucous) surfaces.
- Closed (serous) cavities.
- Tissues of organs or parts. (*Psorospermiæ*, *Cysticerci Nematoids*, etc.)
- Blood. (*Bacteridia* in 'Malignant Pustule,' *Psorospermiæ* in 'pébrine,' etc.)

Apparently caused and propagated by the presence and self-multiplication of living units.

TISSUE DISEASES.

A. *Diseases of Internal Formed Tissues and of Mucous Membranes.*

All inoculable and capable of arising de novo.

- Fibro-plastic growths.
- Cancerous growths.
- Tubercular growths.
- Glanders.
- Syphilis.
- Gonorrhœa.
- Purulent ophthalmia.
- Diphtheria and Croup.

B. *Diseases of the Blood (principally).*

All contagious and capable of arising de novo.

- Erysipelas.
- Puerperal fever.
- Surgical fever.
- Pyæmia.
- Hospital gangrene.
- Rabies.

Principally sporadic.

- Rheumatic fever.
  - *a.* Dengue.
  - *b.* Sweating Sickness.
- Intermittent fever.
  - *a.* Remittent fever.
  - *b.* Yellow fever.

Principally endemic.

Contagiousness either absent, little marked, or more or less virulent; all probably capable of arising de novo.

- Summer diarrhœa.
  - *a.* Choleraic diarrhœa.
  - *b.* Cholera.
- Dysentery.
- Influenza.
- Mumps.
- Relapsing fever.
- Typhoid fever.
- Typhus fever.
  - *a.* Cerebro-spinal meningitis?
  - *b.* Plague.
- Varicella.
- Hooping-cough.
- Measles.
- Scarlet fever.
- Small-pox.

Often epidemic.

Caused and propagated by chemico-physical agencies, and not by the multiplication of living units.

In the first place, we find a group of diseases due to the presence upon or within the body of parasitic organisms. These are partly local and partly general affections, the latter being intensely contagious, and on that account frequently confounded with other general infectious diseases in which living organisms do not occur. These general parasitic diseases are propagated by the presence and multiplication

of living units, whilst those of the next great class are not[1]. The tendency in the former is towards death; the tendency in the latter towards recovery. The non-parasitic infective or specific diseases are also partly local and partly general affections. The local affections are closely allied to other morbid states, such as cancer and tubercle, with which they are not usually classed. Many of these local diseases tend to become general diseases. Similar morbid growths spring up in various parts of the body, and the blood itself becomes affected. They are also more or less apt to spread from individual to individual. All are capable of being generated *de novo*. Such local affections are united by the closest bonds of similarity to the more general zymotic diseases, amongst which all degrees of contagiousness are manifested. The members of the whole series, however, are intimately related to one another; and their mode of propagation is essentially similar, even though the readiness with which contagion occurs is variable. Very many of them are undoubtedly generable *de novo*; and the others are probably also capable of arising 'spontaneously,' though the proof of this, on account of their highly contagious nature, is difficult to establish.

All these latter diseases, therefore, are dependent upon local perverted modes of growth, or upon chemical changes of a definite, though unknown, character taking place in the blood—partly under the influence of general causes, and partly owing to the initiation of chemical changes induced by contact-action of contagious particles or fluids. As with diseases in general, so with these, two sets of factors are

[1] A more complete investigation (since the delivery of this lecture) of the facts known concerning parasitic diseases has led me to make certain important modifications in the view above expressed as to the *rôle* of the parasites in these affections, as may be seen by reference to Chap. xix.

frequently concerned in their production. There are the 'predisposing causes' pertaining to the condition and tendencies (either inherited or acquired) of the individual, and there are the 'exciting causes' or external influences (usual or unusual) at the time operative upon this individual. The combined influence of these causes of disease are often called into play in the production of the infective malady, just as much as they are influential in the origination of non-infective diseases. But predisposing causes may, in conjunction with ordinary external agencies, suffice in some cases; just as, in other cases, the exciting cause or causes may be capable of initiating the affections in the average healthy individual, without the aid of any predisposition.

Unless we entertain opinions of this kind, facts, which are admitted by all, seem quite incapable of being explained—whether having reference to the 'generalisation' of morbid growths within the body, or to the spread of infectious diseases amongst the community. Cancerous particles in the circulation are wholly inoperative in certain individuals in inducing the growth of cancer in distant parts, whilst they are only partially operative in many other individuals, however numerously they may exist. Contact with the contagia of ophthalmia or diphtheria will excite the disease in some persons and not in others. Yellow fever and cholera are 'contagious' only when certain favouring conditions are present to facilitate the operation of the specific poisons of these diseases. Rabies cannot be communicated to certain dogs. Professor Gamgee[1] mentions a case in which a pointer did not contract the disease although it was bitten seventeen times by mad dogs. And even the most contagious affections—those in which the poison is usually sufficiently potent to act upon the average individual—do not seem capable of being communicated to some persons. Do we not see

[1] In Reynolds's 'System of Medicine,' vol. i. p. 717.

individuals fully exposed to the contagion of measles, scarlet fever, and small-pox, who yet fail to contract the disease? Facts of this kind are familiar to all medical men. Sir Thomas Watson has referred to the case of 'an old woman who for years had been in the habit of going from village to village as a nurse; and of nursing a great number of persons labouring under small-pox, which she had never had, and against which she (naturally enough) believed herself proof:' but, he adds, 'at length she was taken ill, and died of small-pox in the eighty-fourth year of her age.' Again, he says: 'In 1845, a lady with whom I am acquainted went through an attack of measles, that disease being prevalent in the village where she was then residing. She had never had the measles previously; yet she had long before personally tended eleven of her twelve children when ill of the same complaint[1].'

Such facts are quite inexplicable in accordance with the vital or 'germ-theory' of causation of these diseases, but they become much more easy to understand in accordance with the views which have just been enunciated. They are, further, thoroughly harmonious with the results of experiments made by myself and others with reference to the causes of fermentation. These results have led me to reject, as too narrow and exclusive, the 'vital theory' of Pasteur, and to adopt the broader physico-chemical doctrines of Liebig, which appear to be harmonious with all the facts. In endeavouring to explain the initiation of fermentation in any particular fluid which has been boiled, we have also to consider the influence of intrinsic tendencies in the fluid, in combination with the exciting or external agencies to which it is subjected. In some cases the intrinsic tendencies may of themselves be potent enough to initiate the process; whilst in other instances the mere contact-

[1] 'Principles and Practice of Physic,' vol. ii. p. 782.

action of an unheated organic fragment combines with weaker inherent tendencies to incite the fermentative process. Fermentations may be associated with the presence of organisms, or they may occur independently. The ordinary zymotic diseases are comparable with fermentations of the latter class[1]; and their several contagia act after the fashion of the mere dead organic fragment upon the fermentable fluid[2].

[1] It is, however, quite conceivable that, in certain cases, the changes in the blood might, in the last stages of the disease, assume such a character as to lead to the evolution of *Bacteria* in this fluid. Such a change, which *may* occasionally occur during life, does undoubtedly occur very soon after death, in some diseases. In two cases, one of rheumatic fever and one of typhoid fever, in which the temperature had gone up to 108–110° Fahr. a few hours before death, I found the vessels of the brain and of other parts of the body containing myriads of *Bacteria* even within forty hours after death, and whilst the temperature of the air had not been over 65° Fahr. The blood was blackish and fluid, the organs were much blood-stained, and, in addition to other marks of putrefaction, bubbles of gas were abundant in the meshes of the pia mater. The blood of such, and of other similar patients examined during life, has never revealed to me the least trace of *Bacteria*. Dr. Burdon Sanderson, moreover, has ascertained that the blood and other fluids of the body do not generally exhibit any zymotic tendencies (see 'Thirteenth Report of Medical Officer of Privy Council'). Some of the *Bacteria* which were found after death, I believe to have been evolved *de novo*, whilst others were descendants of those which had so arisen in the putrescent blood. No other view seems to me to be so tenable as this. The fluids in a pyæmic abscess may occasionally be on the road towards similar results, and, even if no *Bacteria* exist, such fluids might exhibit 'zymotic' properties.

[2] Look, again, at the great moral epidemics which were so prevalent in the middle ages, and which in their most marked form have extended almost to our own times. Here, also, we have a changed mode of action in certain parts of the body, brought about partly by 'predisposing,' and partly by 'exciting' causes. We may read in Hecker ('Epidemics of the Middle Ages,' p. 142) as follows:—'In a Methodist chapel in Redruth, a man, during divine service, cried out with a loud voice, "What shall I do to be saved"? at the same time manifesting the greatest uneasiness and solicitude respecting the condition of his soul. Some other members of the congregation followed his example, cried out in the same form of words, and seemed shortly after to suffer the most excruciating bodily pain. This strange occurrence was soon publicly known; and hundreds of people who had come thither, either attracted by curiosity or by a desire, from other motives, to see the sufferers, fell into the same state. The chapel remained open for some

Some *boiled* fluids are quite incapable by themselves of initiating a fermentative process; but this tells no more against the positive abilities of other fluids, than the fact that certain diseases are unable to spring up amongst a particular community, tells against the circumstance that they do so arise amongst other communities where a number of unhygienic surroundings, previously absent, are also operative in producing the result[1].

Amongst the 'exciting' causes of disease, there must be many which are to us at present utterly obscure. More especially is this the case with epidemic diseases. There are, undoubtedly, 'epidemic influences' concerning which we know scarcely anything, but whose existence is only too surely attested by the history of the great epidemic and epizootic affections. As Fleming says, in his 'Animal Plagues,'

days and nights, and from that point the new disorder spread itself, with the rapidity of lightning, over the neighbouring towns of Camborne, Helston, Truro, Penryn, and Falmouth, as well as over the villages in the vicinity. Whilst thus advancing, it decreased in some measure at the place where it had first appeared, and *it confined itself throughout to the Methodist chapel.* It was only by the *words which have been mentioned* [contagia] that it was excited, and it seized none but *people of the lowest education.* Those who were attacked betrayed the greatest anguish, and fell into convulsions. . . . According to a moderate computation 4,000 people were within a very short time affected with this convulsive malady.' The various signs and symptoms of the malady are then described.

[1] There is, however, a great tendency to draw such conclusions; just as there is a tendency with others to conclude that *Bacteria* do not arise *de novo*, because there is no evidence of such an occurrence when dealing with Pasteur's solution or a few other fluids, different from those in which the process is stated to occur. Let any person, for instance, repeat Dr. Sanderson's thirteenth experiment ('Thirteenth Report of the Medical Officer of the Privy Council') with a strong infusion of hay or turnip, rather than with Pasteur's fluid, and then such results will occur that, from Dr. Sanderson's data, he will have no option but to admit that *Bacteria* do arise *de novo*. It is surprising that such an experiment was not tried in the face of all that has been said concerning the productivity of such fluids. The real laws by which contagion is regulated can never be adequately understood, unless one knows whether the *contagia* with which one is concerned can, under any circumstances, arise *de novo*. This seems to me to be the point which should be first ascertained.

'It has been a matter of common observation from the earliest times, and our history will testify to its accuracy, that wide-spread pestilence in plants, and murrain in animals, have frequently either preceded, accompanied, or followed closely on those visitations which caused mortality and mourning in the habitations of men; showing an identity of causation or affinity which strongly tempts the inquirer to solve the secret of their joint production [1].' 'Causes' of this kind, however obscure, are undoubtedly none the less real. Whilst we may hope, therefore, that increasing knowledge will ultimately enable us to throw more light upon their nature, we may at least feel assured that the efficacy of these 'causes' may be increased or diminished by us at will. 'Exciting' causes of all ordinary severity require to be supplemented by the action of 'predisposing' causes existing in the individual himself before disease can be generated. It is true that we are comparatively powerless to rectify mere individual idiosyncrasies, of the very nature and existence of which we may be ignorant, but they constitute a mere fractional part of the predisposing causes which favour the spread of epidemic affections. These are, in the main, produced in the individual by the operation of the more general exciting causes of disease, such as bad or insufficient food, bad water, and impure air; or they are dependent upon more special causes, such as depressing emotions, excessive muscular exercise, or the occurrence of any unusual amount of degenerative

[1] If additional reasons were needed to enforce the vast importance of the fullest knowledge concerning these diseases, they are not wanting. The same author writes:—'The losses from only two exotic bovine maladies ("contagious pleuropneumonia," and the so-called "foot-and-mouth disease") have been estimated to amount, during the thirty years that have elapsed since our ports were thrown open to foreign cattle, to 5,549,780 head, roughly valued at £83,616,834. The late invasion of "cattle plague," which was suppressed within two years of its introduction, has been calculated to have caused a money loss of from five to eight millions of pounds.'

changes within the body. As Dr. Carpenter pointed out, nearly twenty years ago, in a very able article on the 'Predisposing Causes of Epidemics [1],' these causes are reducible to one or other of three categories:—'(1) those which tend to introduce into the system decomposing matter that has been generated in some external source; (2) those which occasion an increased production of decomposing matter in the system itself; and (3) those which obstruct the elimination of the decomposing matter, normally or excessively generated within the system, or abnormally introduced into it from without.' Now the common characteristic here is that 'any one of these causes will tend to produce *an accumulation of disintegrating azotised compounds, in a state of change, in the circulating current*'; and observation seems to tell us that either of the causes leading to such a result may, when potent, suffice to assist the spread of epidemic diseases, though two or more in combination lead to much more certain results. Much has been done to diminish the prevalence of these conditions—which act only too surely upon the individual in giving rise to 'predisposing' causes of disease—though far more still remains to be done: Happily, however, public attention is now becoming (though slowly) aroused to the importance of pure air, pure water, efficient drainage, and wholesome food, as instruments for maintaining the health of the community.

But let us not be blinded by any narrow or exclusive theories which would teach us that epidemic and infective diseases cannot arise *de novo*. Let us, instructed by a broader survey of the facts, assign no such limits to natural possibilities, and not lightly accept theories which lead to supineness when we ought to be stimulated to exertion. Whilst

[1] 'British and Foreign Medico-Chirurgical Review,' 1853, vol. xi. p. 175.

accepting to the full all doctrines which inculcate the necessity of diminishing the chances of contagion by every available means, let us, full of hope, diligently seek also for the causes which engender even the most contagious of diseases. Prevention of disease is *the* grand end and aim of medicine; if, then, we have learned from the sad lessons of experience that scarlet fever and small-pox are virulently contagious diseases; if, even in ninety-nine cases out of a hundred, or even in a still larger ratio, both of these diseases are acquired by contagion, then is it all the more important that we should strive to ascertain what are the invariable and immediately antecedent sets of conditions, or states of system, which suffice actually to engender these maladies. In such cases knowledge and power are most frequently convertible terms. Next to typhus fever, the most fatal of the infective diseases which occur in this country are scarlet fever [1], small-pox, measles, and hooping-cough. The ravages of typhus in our crowded cities and in our jails have been enormously curtailed —not so much because of its diminished spread by contagion, but rather because we have learned what are the causes which engender it, and are therefore better able to prevent

[1] Mr. J. Netten Radcliffe says (Ranking's 'Abstract,' vol. xli. 1865), 'The Registrar-General's returns of scarlet fever for the whole of England, include two periods of five and sixteen years respectively. The first period extends from 1838 to 1842, and the second from 1847 to 1862 inclusive. The total number of deaths registered from the disease in the twenty-one years was 310,720; the annual average mortality for the whole series of years was 14,796. . . . . The history of the progress of scarlet fever in the metropolis differs from that of the entire kingdom in this, that it shows a great augmentation of the mortality from the disease in the last quarter of a century. The annual average mortality from the malady in London during the past twenty-six years was 83 per 100,000 population. The average varied from 32 in 1841 to no less than 174 in 1863. In the quinquennium 1839–43 the annual average was 78; in the quinquennium 1844-48 it increased to 88; in the quinquennium 1859–63 it advanced to 115. The death-rate of 1863 (174) was more than double the annual average of the twenty-six years, 1838–64.'

its occurrence. Let us strive, then, to acquire a similar knowledge concerning scarlet fever, small-pox, measles, hooping-cough, and other contagious affections, and thus endeavour, in the most efficient manner possible, to check the ravages of these wide-spread and persistently pestilential diseases, which are at all times and seasons undermining the health and cutting short the lives of so large a proportion of the human race.

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